A. Jamme

Miscellanées d'ancient arabe

XV

Washington

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Foreword

This XVth volume of the present series is devoted to seven studies. The first deals with the question of the Gebbanites whose location still remains unknown. The next three sections are devoted to different works by J. Pirenne, viz. two sets of texts and her interpretation of al-ᶜUqlah, respectively. The fifth section is the study of some new Saf texts published by V.A. Clark. The eighth division is reserved to the discussion of F.V. Winnett's thesis on Saf and Tham ᵓtm and ᵓtm ᶜl. The last part is a close look at M.C.A. Macdonald's publication of a Saf text which he claims to be an ABC.

I - The Gebbanites.

A - Pliny.

Pliny (VI, 153) writes that "reliqui in continente a noto etiamnum Autaridae, in montes vii dierum transitus, gens Larendani et Catapani, Gebbanitae pluribus oppidis sed maximis Nagia et Thomna templorum Lxv: haec est amplitudinis significatio" (1). E.Glaser understands the preceding information as follows: Pliny "meldet, Katabaner und Gebaniter wohnen in mehreren Städten (Thomna und Nagia)" (2), and H. von Wissmann deduces from Pliny's passage that "Nagia wird hier merkwürdigerweise von Thomna genannt" (3) and states that Nagia was "wohl die zweitgrösste Stadt von Qatabān" (4). Furthermore, on the basis of Pliny's statement (XII, 63-64)," euehi non potest [viz. the incense] nisi per Gebbanitas, itaque et horum regi penditur uectigal. Caput eorum Thomna" (5), von Wissmann is so convinced that "Gebbanitae" and "Catapani" are identical that he translates "Catapani, Gebbanitae" of Pliny's first quotation as "Die Qatabānen, (nämlich) die Cebbraniten" (4).

Pliny's first quotation apparently gives an <u>ethnical</u> description of the people living further south: "reliqui in continente a noto etiamnum," and then follows the enumeration of four ethnical groups. Contrary to H. von Wissmann, the Gebbanites cannot be the same people as the Catapanites because the assumption that the <u>same</u> ethnical group be mentioned <u>twice</u> and <u>consecutively</u> under two different names cannot be justified, and von Wissmann neither mentions nor elucidates the difficulty. Furthermore, both Glaser's and von Wissmann's interpretation which refers the pericope "pluribus oppidis sed maximis Nagia et Thomna" exclusively to "Catapani, Gebbanitae" is not deductible from Pliny's text where the pericope "Autaridae ... Gebbanitae" is an explicative apposition to "reliqui ... etiamnum;" and it is to this main sentence that the pericope "pluribus ... Thomna" does refer. The short exposé of Pliny's last sentence of VI, 153 may in fact be the only remaining item of a longer descriptive narrative

on SA and also that it became a part of VI, 153 because the lastly listed population, the Gebbanites, had a port called Ocilia in XII, 88 (6). The idea of a port could by association of images have introduced the mention of a promontary or vice versa, depending on what really happened.

The Catapanites are, of course, the Qatabanites, but the three remaining ethnical names still remain unidentified although the Gebbanites were already identified as the Min tribe Gabaᶜān by D.H. Müller who wrote: "In späterer Zeit treten uns die Gabaᶜān als 'Gebbanitae' entgegen" (7). This suggestion, however, does not fit Pliny's information on Ocelis. Apparently, Pliny's informant gives the names not only of four tribes living along a northwest-southeast axis, listed them from north to south, but also of the two cities which he must have heard of most often. Whether Pliny's informant ever saw Timnaᶜ himself is most unlikely because of his statement on "Thomna templorum LXV." This statement is practically impossible to admit because the greatest part of the city would have been occupied by temples only. Since any human being can readily see the difference between a temple and any other building, the informant's preceding statement suggests that he never went to Timnaᶜ himself for it is hardly possible that he would have given orally or in writing such an obvious exaggerated report. Furthermore, the informant's second statement on "maximis Nagia et Thomna" cannot be checked as far as Nagia is concerned because the actual extent of the country the informant had in mind remains unknown.

The testimony of Pliny's informant is strictly limited to the following hearsay: there are four tribes living in the northern hinterland and they have many towns of which the greatest are Nagia and Thomna. Finally, the informant of Pliny's statement in VI, 63-64 reproduced above, most probably was not the person who provided Pliny with the information inserted in VI, 153. Whoever the informator was, the veracity of his testimony on "Caput eorum [= the Gebbanites] Thomna" still remains unsubstantiated.

Summarily, Pliny reproduces extremely sketchy, tendentious reports and hearsays containing one-sided, telescoped summaries on institutional realities foreign to those merchants who spread them all around. It is amazing to see scholars taking for granted those scraps of information as if they were objectively historical, and distorting them and some epigraphical information as well in order to build up theories and syntheses.

B - A.F.L. Beeston.

The question of the Gebbanites became the focus of a short paper by Beeston (8) to which subsequent publications have given an undue publicity although von Wissmann had already in 1977 characterized Beeston's position as "nicht möglich" (9). Unfortunately, von Wissmann fails to support his rejection with either a single reason or any discussion of Beeston's statements. Beeston's paper contains two sections where he successively presents two contradictory theses on the identity of the Gebbanites and his theory on the connection of the Ausarites with SA wsr.

Preliminary remark. - The seven mistakes in the ten lines of "REFERENCES" (p. 8) are a good introduction to Beeston's well-known imprecision and voluntary distortion of the facts which are once more displayed in his paper. The name of the first author is not "Beek, G. van" but "Van Beek, G." (cf. also pp. 5 and 7). — In the lines devoted to Mlaker, correct "B." to "K;" "1953" to "1943;" "Hierodulentexte" to "Hierodulenlisten;" and "Maᶜin" to "MaᶜIn." — In the title of Pirenne's book, correct "sudarabe" to "sud-arabe." — In the title of von Wissmann's book, add "Zur" before "Geschichte."

1 - In the second paragraph of his paper, Beeston states that "scholars such as M. Hartmann (1909: 252) ... asserted the identity of the Gebbanitae with epigraphically attested gb'n" (p. 4). In fact, Hartmann states that "Über den Zusammenhang der Obersippe Gabᵓān mit dem Gabbanitae der Alten spreche ich mich hier nicht aus" (10). Furthermore, it is significative that Beeston never once refers to D.H. Müller who, already in 1881, had set forth the same theory, as pointed out above (p. 2).

2 - According to Beeston, Pliny affirms that the Gebbanites "were a people of the interior and had many towns, the largest being Nagia and Thomna [Hist. nat., vi, 154]" (p. 4). Firstly, correct "154" to "153." Beeston's interpretation of Pliny's exposé is erroneous, as already demonstrated above (p. 1). Furthermore, Beeston states that "Nagia is not certainly identified, but there is a good likelihood that Glaser was certainly right in identifying it with the site known today as Hajar Najā, approximately half-way between Tmnᶜ and Marib" (p. 4). The name "Hajar Najā" does not exist; what exists is Najjāᵓ. Glaser first suggested as certain the identification of "Nagia = [illegible] im Gebiete der Mûrad, circa eine Tagreise von Mârib" (11). Glaser, however, revised his theory as follows: "Neǧaᵓ or Naǧaᵓ (höchst wahrscheinlich identisch mit dem Nagia der alten Autoren)" (12). It is strange that the following authors

repeat only Glaser's first opinion without taking into consideration his second, more cautious wording; cf., e.g., von Wissmann (13) who even goes as far as publishing the following equation, "el-Jūba (Nejā)" (14). The identification mentioned above actually is impossible since there are no ruins at Najjā'.

3 - Beeston breaks into an open door when stating that "the conclusion is clearly that gb'n was a dominant group within Ma'in" (p. 5). He further adds that "this is the simple answer to a question that troubles Pirenne, as to why Qatabanian inscriptions contain no mention of gb'n as such ... to the average non-Minaean, not specially interested as Pliny was in the internal organisation of the aromatics trade, the Gebbanites were just Minaeans" (p. 6).

Firstly, Pliny's alledged special interest could not possibly have altered the nature of the material information he received directly or indirectly from the merchands' reports, and these remain what they actually are, viz. second- and third-hand accounts or hearsays. Secondly, that all Min "were just Minaeans" "to the average non-Minaean" (as Beeston put it) is true, but it is equally true of the Min themselves when referring to their own compatriots as a whole without mentioning individuals, as clearly indicated by VL 9/1-2: kbr/m'nm/btmn'/wm'nm/bkl/tmn' leader of Ma'īnum in Timna' and Ma'īnum in all Timna' (for bkl/tmn', cf. JaSIMB, p. 187A; btmn' = bkl/tmn'). Thirdly, Beeston's reasoning based on Pliny's special interest also works the other way: the merchands, so impressed by the fame of the Gebbanites, could readily have accepted the hearsay of the Gebbanites being in Timna' or could even have simplified the overall picture by putting them there. There is indeed no certainty at all, on the contrary, that those merchants who spread that statement, ever went to Timna' to check the situation for themselves.

4 - With his characteristically confusing mind, Beeston speaks of "Ocilia (Bab al-Mandab)" (p. 5). Cf. already, ScPES, p. 115.

5 - Beeston adds that "the fact that they [= the Gebbanites] had their trading headquarters in Tmn' [cf. Pliny, XII, 64] is perfectly compatible with the fact, attested in the inscription Van Lessen 9 ..., that Tmn' had a Minaean colony" (p. 6). The so-called "fact," referred to by Beeston, is not really a "fact;" it is a mere statement made by Pliny. It is not only unsubstantiated, as stated above, but also disproved by the testimony of VL 9, the most important information of which Beeston refuses to give his reader. Therefore, the second so-called fact suggested by Beeston also is erroneous. In this uniquely important text originating from the Min living in Timna', the author of the text, their leader, declares himself to be a member ... not of gb'n, but of the clan

ayn Rawyān, and this clan, which was not a subdivision of gb'n, is well-known in the inscriptions from the country of al-'Ulā. Furthermore, among the seven Min who supervised the building of the burial ground for the Min group, only one, a certain 'slmm (l. 3) belongs to ẓlwmn, which is a subdivision of gb'n. Indeed, if, as Pliny claims and Beeston maintains, Timna' was the headquarters of the Gebbanites, how can Beeston explain and justify that the kbr leader of the Min group was NOT a Gebbanite himself? The truth of the matter is this: the epigraphical fact - and this is not a speculation based on a dream - is that only one out the eight Min persons listed in the inscription belongs to a subdivision of the clan gb'n, and this reality in turn proves not only that the Min colony did not belong to gb'n but also that gb'n was not after all the all-powerful clan in the international affairs of SA, as Beeston would have us to believe.

6 - Beeston pursues his point by stating that the "Minaean colony [in Timna'] (perhaps one might say 'factory') [was] substantial enough to be organized under its own kabīr ..., just like the Minaean colony at al-'Ulā/Dedan in north-west Arabia" (p. 6). Even according to Beeston himself, that Min colony could merely have been a single building! How, then, is it possible to state that it was "just like" Dedan? Dedan was a whole city founded, thoroughly dominated and organized by Min, viz. Dedan was a Min city contrasting with the tiny Min enclave in Timna'. How could such a tiny enclave be called a "capital"? Where is the inscription or the Greek or Roman narrative calling Dedan a "capital"? Furthermore, the word kbr leader was borne by the head of a group of Min and does not at all by itself give any idea on the number of the persons composing that group. What is the real meaning of "substantial" in Beeston's text? His own suggestion of a "'factory'" indicates that, even in his opinion, the number of those Min could not have been "substantial" at all. However, regardless of the actual size of the group, those Min had to have a chief, and his title was kbr. For this leader, the actual number of persons under his command, whatever it might have been, was largely compensated by his being the official representative of his country in a foreign land.

7 - Apparently dissatisfied with his own unsubstantiated, vague and imaginative idea of the Min 'ahlān being "kinship groups on a structural level with the central Arabian ḥayy, and a survival from their nomadic past," a theory which is wholly irrelevant to the case at hand, Beeston drops very quietly without saying it explicitely of course the theory on the Gebbanites being the Min clan gb'n, which he had spent so much time and energy to promote and he now suggests

that the Gebbanites were not the Min clan gbʾn after all. He writes this:

> "I would suggest therefore that, while the original nucleus of Maᶜin, and the builders of Rṣfm, may have been of the nature just described, this nucleus had in time linked up with other groups engaged in South Arabian trade, to form what can only be described as a sort of Hanseatic League, aimed at monopolizing the entire trade of the south-west Arabian culture area; and that the ʾahālī of Maᶜin were not kinship groups but component members of the League from all over the culture area, some of whom prospered greatly in their commercial undertakings, some less so."

It is quite remarkable, in my opinion, that no matter what opinions Beeston presents, no matter how close they follow each other, whether contradictory or not, Beeston always appears to propose them so attractively and to illustrate them so enticingly that the reader cannot but marvel at the chameleonic versatility of his restless imagination. In the present case, he seems so blinded by his natural urge ((he most probably would call it "my instinct;" cf. JaMAR, XIV, p. 80)) to interlard his argumentations with comparisons involving other cultures that he could not even see the incongruity of mentioning the Hanseatic league for this league was composed of members belonging to the same ethnical group in their dealings with foreigners. Furthermore, he does not seem to be aware of the strangeness of his new theory, viz.

a MIN "league" (the idea of a "league" remains gratuitous and unfounded epigraphically)

completely dominated by a NON-MIN member (whether this group was SA or not, Beeston does not say) and

having its headquarters in the capital city of QATABÂN!!!

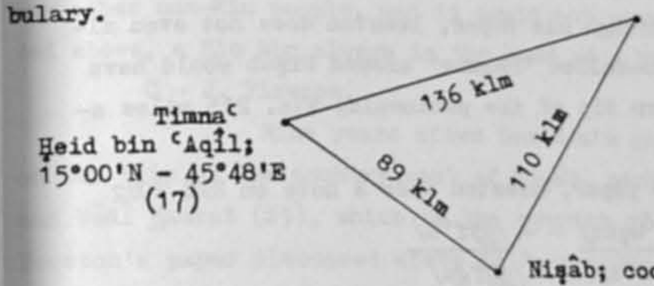
8 - The final touch of Beeston's presentation reads as follows:

> "There is an important area inscriptionally attested as Wusr (and the name survives to the present in Ḥuṣn Wusr), centred on Niṣāb and substantially to the east of Baiḥān; indeed Niṣāb is practically equidistant from Tmnᶜ and Shawa - half-way between them if one pursues a route keeping to the foot of the mountains, thought there is admittedly a more direct Tmnᶜ - Shabwa route acros [sic!] desert. The ESA plural meaning 'people of Wusr' would have had the form 'Awsār; and I think that Wusr is at least as likely a candidate for being the regnum of the Gebbanites as any other locality" (p. 7).

This long quotation is yet another good example of Beeston's literary production containing grossly distorted facts.

a - It is hardly necessary to point out the fallacy of "inscriptionally attested as Wusr" since SA inscriptions are unvocalized, and the vocalization of modern Wusr does not, and cannot, adequately prove that "Wusr" was also the pronunciation in pre-Islamic time.

b - The statement according to which that area is "centred on Niṣāb" is another distortion of the facts. In modern time, Niṣāb = ʾAnṣāb is the most important town in the area. But it is equally certain that, if the area was called wsr in SA inscriptions, that name comes from the wâdî itself which is still today called Wâdî Wusr, as the areas of Baiḥân and of Ḥaḍramawt bear the names of the wâdîs which cross them. Present-day Wâdî Wusr and the town Wusr on the eastern side of the wâdî, the coordinates of which are, respectively, 14°15'N - 46°11' E and 14°18'N - 46°12' E (15), are about 25 miles (as the crow flies) southwest of Niṣâb, and this town cannot be considered as the center of the SA area of wsr. It, thus, follows that the SA area of wsr is not, as Beeston claims, "substantially to the east of Baiḥân" as Wusr and Wâdî Wusr are latitudinally about 17 miles south of the southernmost tip of Wâdî Baiḥân. Even, the town of Niṣâb is latitudinally about 2 miles (in a straight line) south of the southernmost tip of Wâdî Baiḥân. Therefore, the SA area of wsr is definitely southeast of Wâdî Baiḥân. Furthermore, that "Niṣâb is practically equidistant from Tmnᶜ and Shabwa" is yet another example of the looseness of Beeston's vocabulary.



How can anyone speak of "substantially equidistant" positions when there is a difference of 20 °/₀ between the two distances? Furthermore, that so-called "practically equidistant" becomes "half-way" as if the "route keeping to the foot of the mountain," instead of greatly lengthening the distance, was reducing the 20 °/₀ difference to zero. Did Beeston himself walk that famous "route," while pushing an odometer, to prove his point? Moreover, no caravan from Šabwat and heading for Timnaᶜ would have to go as far down as Niṣâb; it would go either through the sesertic area to Timnaᶜ or take a southwesterly direction and use a wâdî, such as Wâdî Rumḥ.

c - Beeston's suggestion is contradicted by Pliny's VI, 153, which is reproduced above (p.1) according to which the Autarides are different from the Gebbanites.

d - Wusr, which is described seven lines above (in Beeston's text) as "an important area," is now considered as a "locality." Wusr obviously is a substitution for Niṣâb.

e - Beeston fails to explain why, of all the names mentioned by Pliny, the name "Ausaritis" would be derived from a (non-epigraphically attested) gentilic plural "ʾwsrn" instead of from the name of a country.

SA qtbn is mentioned as "Catapani" (VI, 153);
ḥḍrmwt is mentioned as "Atramitae" (XII, 52); and
mᶜn is mentioned as "Minaei" (XII, 58).

Why is wsr not mentioned as "Wasritae" or something similar?

f - Here again, Beeston fails to mention an obvious discrepancy. On the one hand, as stated above (p. 1), the Autarides are different from the Gebbanites, according to Pliny, VI, 153. On the other hand, two kinds of incense apparently come from the Gebbanites' "regnum" according to Pliny, XII, 69 (19): "Gebbanitica et Ausaritis Gebbanitarum regno." Beeston would identify the Ausarites with the Gebbanites. It should be recalled here that von Wissmann had already suggested a similar idea for "Catapani" et Gebbanites.

g - All through his paper, Beeston does not even allude to the main difficulty, viz. that so-called "regnum" around Niṣâb would have a port located in the extreme southeastern tip of the peninsula, viz. 215 miles away, as the crow flies.

9 - At the end of his paper, Beeston adds a note on RÉS 3022 (p. 8). The text starts as follows (1): ᶜmṣdq/ - - /ḏyfᶜn/
wsᶜd/ - - /ḍḍfgn/
kbry/mṣrn/wmᶜn/mṣrn/ -

a - Beeston starts saying that "in ESA idiom this could mean that one [of the two authors] was kabīr of one place, one of the other" (20). This statement is erroneous inasmuch as it reflects the preconcieved, erroneous idea that kbr always introduces a place name (cf. below, III F x 4, where W.W. Müller claims that it introduces a tribe name). Then, Beeston refers to RÉS's interpretation of both mṣrn and mᶜn/mṣrn, which reads as follows:

"mṣrn est la région dans laquelle se trouvait la colonie minéenne d'El-ᶜEla, au nord-ouest du Hedjaz" and
"mᶜn/mṣrn est vraisemblablement la ville de Maᶜân, dans la même région" (21).

These two identifications are unacceptable because the country of al-ᶜUlâ belonged to the Liḥ, not to the Midianites, who lived east of the eastern arm of the

Red Sea, and Maᶜân, a city northeast of al-ᶜAqabah and about 470 klm (as the crow flies) northwest of al-ᶜUlâ, cannot be seen in mᶜn because such a theory would gratuitously presuppose another Min colony in the far north of the Arabian peninsula. As stated in JaSIMB (p. 73A), "mṣrn is a proper name, but could have originally meant 'the expeditionary corps'" of the Min in northern Arabia; and mᶜn refers to the Min merchants, etc., who lived there under the protection of that expeditionary corps.

b - Beeston then affirms that "the phrase [= mᶜn/mṣrn] looks very much like a parallel to mᶜn/btmnᶜ 'the Minaeans in Tmnᶜ' and thus meaning 'the Minaeans of Midian'." The two expressions cannot be considered as parallels because the local preposition does not exist in mᶜn/mṣrn, and elsewhere in the texts, ll. 3 and 4 (thrice in each line), mᶜn means Maᶜîn.

c - Beeston finally spells out the pearl of his theory which is the fanciful consequence of his argumentation: "This would seem to imply in consequence that the kabīr of Mṣrn was the head of other-than-Minaean Midianites." Such a statement affirms exactly the opposite of the epigraphical information. The title of the two leaders does not speak of "Midianites" and/or of any other non-Min people, and it could not possibly do so since, as already stated above, a Min kbr always is the head of a Min group.

C - J. Pirenne.

Nine years after Beeston's paper, Pirenne released hers, PiPH, on her trip to the country west of Niṣâb, centered around Wâdî Hajar/Ḥijr (22) and Wâdî Ḥawrat (23), which is the country of ancient wsr. According to Pirenne, Beeston's paper discussed above is "une étude convaincante" (p. 222), viz. the "Gebbanites étaient effectivement un groupe minéen: les GBʾn des inscriptions; ils avaient un rôle international en monopolisant le commerce des aromates" (p.224). She differs from Beeston in thinking that Wusr was not "centred on Niṣâb," as Beeston as suggested (cf. above, pp. 7-8), "la capitale commerciale de 'Awsan" "pourrait être alors" "Hajar Yahar" (p. 223), about 23 klm north-northwest of Niṣâb. Although possible, this opinion is by no means certain at the present time.

The two main suggestions contained in Pirenne's paper are as follows.

1 - The kbr of the Min colony in Timnaᶜ is "sans doute le 'roi des Gebbanites' dont parle Pline, et auquel on payait la taxe sur la myrrhe, à Timna'" (p. 224).

As already noted above, p. 1, Pliny's text is "euehi non potest nisi per Gebbani-

tas, itaque et horum regi penditur uectigal. Caput eorum Thomna" (XII, 63-64). The value of this text has already been discussed above, p. 1. Furthermore, the link between the kbr of the Min colony at Timnaʿ and the "horum [= the Gebbanites] rex" referred to cannot be asserted, as already noted above (p. 5), that kbr belonged to a clan different from gbʾn, and that the proportion of the people of gbʾn in that Min colony at Timnaʿ is so small, ONE OUT OF EIGHT, that gbʾn could not have been that all-powerful clan in international affairs, as claimed by both Beeston and Pirenne. Last, but certainly not least, who could believe that the king of Qatabân would have allowed foreigners to independently levy taxes on incense in his own capital? This is impossible.

2 - Pirenne suggests that "le terme 'royaume' est utilisé par Pline (ou sa source) dans le sens où Strabon parle des 'cinq royaumes' qui se partagent l'Arabie Heureuse" (p. 225).

a - The reader notes that Pliny's text speaks of a "rex," not of a "royaume," a word which Pirenne needed to make her parallel perfect. Yet, "rex" does not necessarily refer to a kingdom; it can also designate a head, a chief, a leader, and a master. Therefore, even from the start, Pirenne's parallel does not exist.

b - Pirenne refers to her previous (24) study of Strabo's text, which may be summarized as follows: South-Arabia is divided in "'cinq royaumes', qui semblent avoir été compris par Strabon comme cinq classes de populations" (p. 134); according to Miss Préaux, "le mot βασιλείας pourrait avoir remplace ἔθνη , pris au sens de 'peuples' ou 'tribus' ... Il y aurait donc eu, selon l'information originale, cinq ἔθνη 'corporations'" (p. 135); and the fourth and fifth "classes de populations" are "le quatrième produit la myrrhe et le cinquième l'encens" (p. 127A).

The idea of "corporations" in unacceptable in SA. Furthermore, the crux interpretum is the following pericope πενταχῆ σχίζουσιν εἰς βασιλείας which is translated by JoGS as "is split up into five kingdoms" (25). The Greek text shows that the enumeration of the five divisions is composed of two very dissimilar sections. The first three sections are social classes of human beings which can be called ἔθνη ; the three words involved are in the plural, viz. μαχίμΟΥΣ, γεωργΟΎΣ, βαναυσοτεχνοῦντας.

Each of the last two divisions is an adjective in the singular with the substantive γῆ earth, land understood (26), viz. σμυρνοφόρΟΣ bearing myrrh,

and λιβανωτοφορΟΣ bearing incense; these two

sections cannot refer to two of the "cinq classes de populations" or of the five "corporations;" they refer to parts of the land, and Pirenne's translation of the Greek words as "le quatrième produit la myrrhe et la cinquième l'encens" is erroneous, beside the fact that her rendering of ἡ and ἡ as "quatrième" et "cinquième" is not a traduction, but a commentary.

It is very difficult to justify Strabo's presentation of τὴν Εὐδαίμονα : even considered from the view point of economics, which is evident, his division is grossly incomplete because no mention is made of any of the other social classes which also intervened in the economy of the land, viz. the administration of the "kingdoms," the temples and their peoples, the slaves, the servants and the needy. Even, from the restricted viewpoint of the incense trade, there can hardly be any doubt that the temples were the largest consumers of incense. Yet, they cannot be included or understood in any of Strabo's fivefold division, even if each of the five words is given the widest possible meaning; viz., e.g., μαχίμους designates the fighting men, but could also include their families, their servants as well as the whole hierarchy of military officers; σμυρνοφόρος refers to the land itself, but could also include all the personnel (and all their families and servants) in any way connected with the production, the exploitation and the trade of the myrrh.

On the one hand, it does not really matter whether Strabo's pericope exactly reproduces the information he received (orally or in writing) or is of his own composition based on that information. It is impossible to determine what really happened. However, it is beyond any doubt that he would not repeat under his own name or have himself written statements which good common sense must reject out of hand. Indeed, any translation based on the ordinary meaning of βασιλεία , such as JoGS's, is obviously unacceptable because the five divisions contained in Strabo's pericope cannot EITHER be called "kingdoms" or "tribe" or people or "corporations" OR be considered as the objective description of the whole SA, OR be characterized as "social classes" since the last two of them represent each a geographical section of the land. It is, therefore, necessary to look somewhere else for possible clues leading to a satisfactory translation.

On the other hand, Strabo's pericope is further singled out by the two following characteristics.

a - It contains TWO hapax legomena, viz. βαναυσοτεχνουντας and σμυρνοφόρος (27).

b - The verb σχίζω is followed by the preposition

εἰς . JoGEL does not list a single case of verb + prep. indicating the result of the division; the only example he quotes is ἔσχισε δώδεκα μοίρας , i.e. divided them into twelve parts" (p. 1746A).

All the preceding remarks make it possible and, in my opinion, justify the suggestion according to which the expression σχίζουσιν εἰς βασιλείας may also contain other words used with an unfrequently attested meaning. In fact, I should like to suggest the following points:

a- the expression εἰς βασιλείας may be considered as a modal adverbial locution qualifying σχίζουσιν and specifying the basis upon which the division was done;

b- εἰς is also found "to express RELATION; towards, in regard to, in respect of" (28); and

c- βασιλεία is also attested once as "Pass., being ruled by a king" and also as a substantive, "tiara, diadem" (29).

Therefore, I should like to suggest translating the beginning of Strabo's phrase κατ' ἄλλην δὲ διαίρεσιν σύμπασαν τὴν Εὐδαίμονα πενταχῆ σχίζουσιν εἰς βασιλείας as follows: "And, according to another [economical] distribution, the whole (30) South-Arabia is divided into five sections from the crown's point of view."

D - N. Groom.

1 - Six years ago (31), Groom devoted a whole paragraph (28 lines) to the presentation of Beeston's theory with no critical remark of any kind (pp. 179-180); just an offhand rejection limited to this single phrase: "Beeston's suggestion remains no more than an idea which ... awaits further discovery before it can be proved or disproved - another example of how scholars must conjecture until archaeologists have explored south Arabia more fully with their spades" (p. 180).

No archaeological discovery is needed to see how distorting Beeston's presentation of the facts really is.

2 - Groom misunderstands Pliny's exposé by saying that Timnaʿ is "one of the towns of a people called the Gebbanites" (p. 181). He further states that the distance between Šabwat and Timnaʿ is "110 miles" "via Aiyadim" or "4-5 day's journey" (p. 213: "Table Showing Distances and Journey Times on the Incense Road"). These figures must obviously enough be considered as mere approximations, as the author's following schema proves it:

his book	PSAS, 6 (1976), p. 71
"4-5 day's journey"	"five or six days"
"110 miles"	"about 110 miles

3 - Groom utilizes Pliny's information mentioned above, but does not refer to either WiZAG, where the question is dealt ex professo, or WiGS, I; nor does he list these publications in his bibliography (p. 274). The author speaks of "a ruin site called Hagar Nagā. It is supposed that these ruins, partly covered by another village, may mark the site of the city of the Gebbanitae called 'Nagia' which was recorded by Pliny" (p. 184) (32).

Here again, the preceding confusion is clear: the expression "Hagar Nagā" comes from WiHöBGS (pp. 36-37) where the ruined city is said to be located "umweit Nejjā" (p. 36), viz. near Nejjâ'. As stated above (pp. 3-4), that "Hagar Nejā" is al-Hajar. Furthermore, that city was not "the city of the Gebbanitae." von Wissmann's theories are dealt with in my forthcoming publication of the inscriptions of al-Jûbah.

4 - Groom's book should be utilized with the greatest caution because of the unreliability of its presentations and summaries (33). Three examples suffice here.

a - The question of the date of the Periplus is dealt with one-sidedly. The two basic papers were not utilized and are missing from the bibliography, viz. J. Pirenne's long article (34) and M.G. Raschke's extensive and detailed rebuttal of Pirenne's theory (35).

b - Groom reproduces ScPES's translation of the Periplus, 19-36 (pp. 90-94) and, as he claims, "with a few corrections (in italics) proposed by Professor Beeston" (p. 89). In fact, those so-called "few corrections" concern a single relative proposition of par 19, and Beeston's so-called "corrections" are nothing but the English rendering of J. Pirenne's original French translation. The passage involved is translated as follows:

by ScPES (p. 29, par 19)	by Pirenne (36)	by Beeston (cf. Groom)
"from which there is a road to Petra, which is the subject to Malichas, king of the Nabataeans."	"par où il y a vers Pétra, chez le roi Malichas, une montée."	"from which there is a way to Petra to the court of King Malichas."

c - Groom offers his reader his own fantastic, grossly erroneous version of our escape from Mârib on February 12, 1952. He writes:

"The American expedition to Mârib was short-lived, because local hostility developed against it until its member were driven out at gun-point, their equipment and finds being abandoned" (p. 185, the underlining is mine) with reference to note 24 which reads as follows: "An account of this expedition and its explusion [sic!] is given by Phillips, 1955" (p. 257).

(1) The expression "local hostility" is ambiguous and tendencious. As the member of the expedition who spent the longest time in Mârib in 1951-1952, I have to say that the local population and even the soldiers (taken individually or acting on their own) never ceased from being very sympathetic toward the expedition. Only the local authority personified by the ʿAmîl ʿAbdulraḥmân al-Yusufî and the two representatives of Prince Ḥassan of Ṣanʿâʾ was opposed from the start to the work of the expedition. The attitude of the ʿAmîl was conditioned by that of former ʾImâm ʾAḥmed, whose change of mind is the subject of a note published in JaMAR, XII (1982), pp. 57-59. The main statement of that note reads as follows:

"Imâm Aḥmed's change of mind was the result of the intervention of Aḥmed Fakhry and his friends in January 1951 when they went to Taʿizz and succeeded in convincing the Imâm that the Mârib excavation campaign should be brought to an end."

(2) The statement according to which "its members were driven out at gunpoint" is grossly mendacious. The opposite is true: we were not "driven out" at all; we escaped; ʾImâm ʾAḥmed did not want us to leave. When informed that we had escaped, he ordered his soldiers stationed in Ḥarîb to intercept us before crossing the Wâdî Beiḥân border. Before the local authority in Mârib realized that we had escaped on the sly, we had fortunately gained enough precious time on our flight toward the border that we crossed the road coming from Ḥarîb while still beyond the ordinary rifles of the party of oncoming soldiers.

(3) The expression on "their equipment being abandoned" is also tendentious and erroneous. The expedition never had the desire of taking away from Mârib and Yemen any finding unearthed during the excavations. From the beginning of the APSM work in Mârib till the time of my house arrest ordered by ʾImâm ʾAḥmed, all valuable loose objects found in the entrance court of the Maḥram Bilqîs were duly transported to the expedition headquarters and stored in a special room of the compound; the soldiers kept that room under constant surveillance. The door of that room was locked when we departed. From the beginning of my forced inactivity till the day of our escape, the removal of all loose objects to the museum could not be carried out normally due to the daily harassment and limitations imposed upon the expedition by the local authorities; therefore, some objects, viz. inscribed stones, were left on the ground inside and outside the excavation field. Groom's choice of the verb "abandon," which means "to cease to assert or exercise an interest, right, or title to esp. with the intent of never again resuming or reasserting it" (37), is slanderous. The expedition NEVER AT ANY TIME relinquished its interest or right to use those findings for the purpose of research; therefore, anyone who has in the past published any inscription or any material unearthed by the APSM members in Mârib, and / or anyone who will in the future do it, are guilty of scientific theft. The same verdict applies also to the expedition's "equipement" or any part of its property, such as the four cubic meters of later squeezes stored in my bedroom at the time of our escape because the expedition has never at any time relinquished its ownership rights on any of those items. Summarily, APSM has never "abandoned" and still asserts its basic ownership rights on any item that either was its own before and during the excavation campaign or has become its own as the result of its work. Similarly, the members of the expedition never relinquished their ownership rights on their personal belongings which they could not take alongwith them.

E - D. Doe.

1 - In his first book (38), Does first quotes (p. 70) RaPNH's translation of Pliny, VI, 153 (p. 453), which he ascribes to "VI, 151-154." In his quotation , however, he omits the comma before "of which" and, much more importantly, he starts his text with "The Gebbanitae," thus eliminating the three preceding ethnical names. Subsequently, he endorses von Wissmann's interpretation by mentioning "Gebbanitae (Qataban)" (p. 123) with reference to note 151: Pliny,"XII, 87-9" (p. 253A). The text of par 88 does not prove at all his identification of the Gebbanites with Qatabân for Qatabân is not even mentioned there (39).

2 - In his second book (40), as everyone was expecting, Doe quietly casts off his allegiance from von Wissmann to Beeston whose theory is summarized as follows: the Gebbanites were Min; they had "several towns of which the largest were Nagia and Thomna (Timnaʿ);" they "lived and conducted the incense trade from their headquarters there"[= Timnaʿ] (p.99), and "Wadi Wusr, the probable location of the Awsarites, and later probably the area of the Regio of the Gebbanites, the Gabʾân of Maʿin" (p. 100).

3 - The accuracy is definitely not the trademark of Doe's publications; this is further exemplified by the following excerpts.

The northern building excavated in Timnaʿ by AFSN in 1950-1951 is called in his book "the temple" (p. 62), or
"the main temple" (pl. 105-108), or
"the temple or religious sanctuary" (p. 219, fig. 36), or
"generally known as the temple of ʿAtar" (p. 218; of course, the divine name is ʿAttar as in p. 262B).

The same building is called in his second volume as follows:

"religious structure. ʿAthtar?" (p. 158), or
"the so called [sic!] temple of ʿAthtar" and "the main religious structure" and "generally known as the 'temple' of ʿAthtar" (p. 172, text), or
"the 'temple' or 'palace' structure" (p. 172, fig. 56a), or
"the temple of ʿAthtar" (p. 173, text), or
"the so-called temple" (p. 173, fig. 56b, and p. 174), or
"temple, ʿAthtar" (p. 233), or
"temple or palace" (p. 237).

4 - Doe's method of argumenting is further illustrated by the case of the walls which, according to him, are "lined internally with marble slabs and votive inscriptions... Some of these were in pairs" (p. 174) without any detail or reference of any kind. Such a statement is baffling because my field book does not mention anything of the kind. In note 47, however, which is connected with "a marble pavement," Doe refers to "van Beek, 1952, 12" (p. 181). It is hard to understand that Doe still does not know (cf. also p. 264) how to spell the name of "Van Beek." But, although having nothing whatever to do with "marble pavement," the second half of that note 47 reads as follows: "For one of a pair of texts see B.M. 132333 or Louvre. AO.21.124" (p. 181).

The most important remark to be made is that Doe is making up the story of the relation of those two texts with the northern building of Timnaʿ. The caption below the photograph of BM 132333 is "Limestone Votive Stela from Hagr Ḥimū al-Zarīr" (41) and

the second slab was purchased from the villagers of Hajar Kohlân (42). Pirenne's idea that VL 1 "est étrangement semblable à " AO 12.124 ... Les dimensions ... correspondent" is fantasy since the bottom of VL 1 is missing; cf. JaMAR, II, p. 44 on BM 132529: "Face antérieure: 31.5 x 44 cm. (maximum)." Therefore, it is another phantasy that those two slabs "aient dû faire partie du revêtement mural du même temple."

Finally, Pirenne does not know where AO 21.124 came from for she states "vraisemblablement à Timnaʿ."

Although the end of Doe's study is an attempt at giving the reader the definitive impression that the building was a temple even with the help of a pure invention on the origin of the two above-mentioned stones, the last phrase of p. 174 leads unexpectedly into another direction, viz. "This building should be compared with the 'Palaces' of Shabwa."

Since the reader does not find any reference to any publication where the idea of a "palace" instead of a "temple" was suggested or where the so-called "palaces" of Shabat are mentioned, he cannot but think that the idea originates from Doe himself. In fact, I already stated more than thirty years ago (and eighteen years before Doe's first volume) that "toute cette bâtisse est une construction royale" (43). More recently, JaMAR, IX (1979, thus three years before Doe's second volume, pp. 3-10) gives all details known to me in support of my original interpretation of the building as "the throne house of the king Šahar Yaġul Yuhargib" (p. 10). As could have been expected, those two publications found no place in Doe's bibliography (p. 271).

Notes

(1) Cf. RaPNH, p. 452.
(2) Cf. Die Abessinier in Arabien und Afrika, München, 1895, p. 113.
(3) Cf. WiZAG, p. 103. (4) Cf. WiGS, I, p. 108. (5) Cf. ErPAHN, p. 39.
(6) Cf. "in portum Gebbanitarum qui vocatur Ocilia" (cf. ErPAHN, p. 48) = Ocelis (cf. ScPES, p. 114, 25).
(7) Cf. Die Burgen und Schlösser Südarabiens nach dem Iklîl des Hamdânî, Vienna, p. 1027. (8) Cf. PSAS, 2 (1972), pp. 4-8.
(9) Cf. WiGS, I, p. 109, note 231.
(10) Cf. Die arabische Frage mit einem Versuche der Archäologie Jemens, Lepizig, 1909, p. 252, ll. 3-5.
(11) Cf. Die Abessinier..., p. 115.
(12) Cf. D.H. Müller und N. Rhodokanakis, Eduard Glasers Reise nach Mârib, Vienna, 1913, p. 132B.
(13) Cf. WiHöBGS, p. 64. As it shall be proven later, von Wissmann is the sole responsible for all geographical and historical theories published in WiHöBGS.
(14) Cf. l.c., p. 42. (15) Cf. GaPDRY, p. 199B. (16) Cf. l.c., p. 176A.
(17) Cf. l.c., p. 89B. (18) Cf. l.c., p. 152A. (19) Cf. ErPAHN, p. 41.
(20) Beeston repudiated his opinion in JSS, 20 (1975), p. 270 and note 1.
(21) Cf. RÉS, V, p. 306.
(22) The coordinates are "14°30'N - 46°22'E" (cf. GaDPRY, p. 90B).
(23) The coordinates of both the wâdî and the town are "14°26'N - 46°09'E" (cf.

(cf. GARBINI, p. 121B).

(24) Cf. Le royaume sud-arabe de Qatabân, Louvain, 1961, "p.126 suiv.;" pp. 134-135 are most important.

(25) In Bible et Terre Sainte, no. 177, janvier 1976, p. 18C, J. Daoust translates the word as "States."

(26) Cf. JoGRL, p. 1620B. JoGS's edition translates the two words as "the myrrh-bearing country" and "the frankincense country," respectively.

(27) Cf. JoGRL, pp. 305B and 1620B, respectively.

(28) Cf. L.c., p. 491B. (29) Cf. L.c., p. 309A.

(30) The Greek adjective σύμμικτον is not translated by JoGS, p. 365.

(31) Cf. Frankincense and Myrrh, London, 1981.

(32) Groom had already stated the same opinion in PSAS, 6 (1976), p. 75: "The village of Nagā, with Hagar Nagā which we suppose is Pliny's town belonging to the Gebbanites called Nagia." That "we suppose" becomes a reality in the map (p. 70) where the location of "Nagia" is certain.

(33) JaNAR, XIV (1985) is my last publication in date in a long series where obsequious flatteries between present-day authors at the expense of scientific objectivity are recorded and denounced. Groom's book offers another twofold example of that kind of egoistic political manoeuvering. According to J. Rodinsons, Groom's book is "un excellent traitement d'ensemble - dont l'absence se faisait vivement ressentir - de tous les problèmes des aromates sud-arabes et leur commerce dans l'antiquité" (cf. Académie Royale de Belgique. Bulletin de la Classe des Lettres et des Sciences morales et politiques, 5e série, 67 [1981-5], p. 254, note 2). Without wasting any time, M. Rodinson quotes the preceding flattery - but he omits the incidental phrase set between the two dashes without indicating its place in the text - and adds "selon le jugement compétent de Jacques Ryckmans" (cf. J. Chelhod et al., Le peuple yéménite et ses racines, Paris, 1984, p. 88).

(34) Cf. JA, 249 (1961), pp. 441-459.

(35) Cf. H. Temporini and W. Haase, Aufstieg und Niedergang der römischer Welt, Berlin, 1978, pp. 663-666 and corresponding notes.

(36) Cf. Le royaume sud-arabe..., p. 189.

(37) Cf. Webster's Third New International Dictionary of the English Language Unabridged, Springfield, MA, 1976 or 1981, p. 2A.

(38) Cf. Southern Arabia, London, 1971.

(39) Cf. ErPAHN, p. 48. (40) Cf. Monuments of South Arabia, London, 1983.

(41) Cf. Cf. JNES, 21 (1962), pl. 5 after p. 38.

(42) Cf. J. Pirenne, in CIASA, Louvain, 1977, p.125.

(43) Cf. Oriente Moderno, 33 (1953), p. 142.

II - Notes on the inscriptions found by J. Pirenne west of Niṣâb.

1 - Pirenne's paper, PiPH , on her trip to the country of Niṣâb, centered around Wâdî Ḥajar, has already been utilized in the question of the Gebbanites (cf. above, pp. 9 foll.). The author publishes a few inscriptions in both cursive and monumental alphabets in such an offhand manner that it would have been difficult to mistreat them more openly except by simply ignoring them.

2 - A characteristic of Pirenne's paper which appears even at first glance, is the messy, constant confusion of her transliteration system of the Ar proper names. Is it really so difficult to adopt a single transliteration all through the pages of a paper? Is it not rather an unmistakable proof of the inconsistency of an unstable mind which, apparently unable to follow a straight line, wanders restlessly? The examples are so numerous that it would take several pages to list all the examples. Here follow four of them:

"wādī Girdān" (p. 213) = "wadi Ǧirdān" (p. 241);
"Biʾr al-ʿAwja" (p. 215) = "Bīr al-'Awga" (p. 216);
"wadi Ḥager (ou Ḥigr)"(p. 213) = "wādī Ḥager" (p. 214) = w. Ḥager" (p.215) = wadi Ḥager" (p. 217); and
"wādī al-Ghayl" (p. 214) = "w. al-Ghayl" (p. 215) = wādī Ghayl" (p. 221).

The reader noticed, I am sure, the two transliterations of "wadi" and "wādī."

3 - Ja 3171: the boulder bearing the present inscriptions, is located "à 1,5 mille en amont de Ganadila" (p. 218).

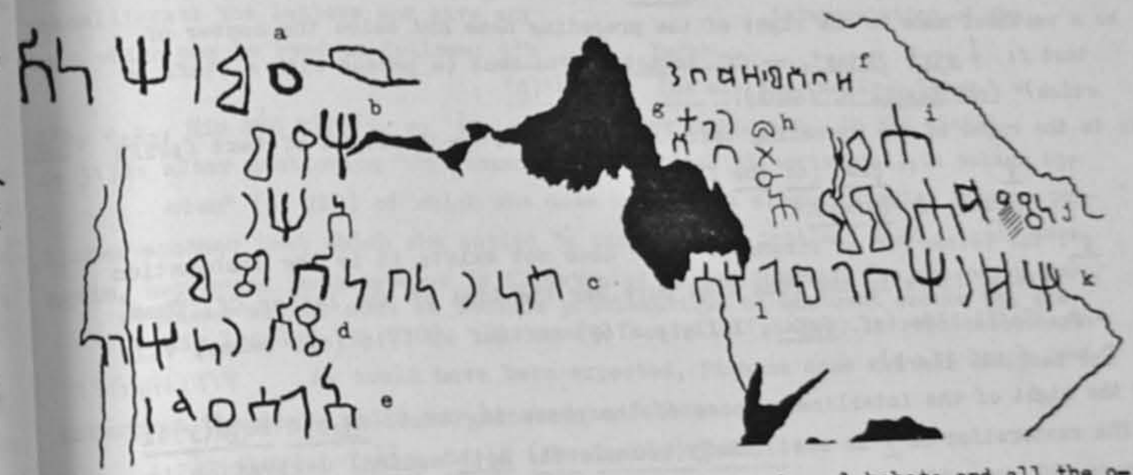
The preceding name is also listed as "al-Ganadila" (p. 214) and "al-Ǧanadila" (p. 215); similarly, the reader finds "wādī Ganadila" (p. 218) = "wādī Ǧanadila" (pl. Va). — The boulder is described without any reference to the photograph of pl. Va, as follows: "un énorme rocher est couvert d'inscriptions. Ce sont des noms, en très ancienne écriture sabéenne, oblitérée par de l'arabe (à gauche), deux ou trois graffites 'thamoudéens' (a droite), avec une inscription en grandes lettres, d'époque monumentale, d'un style apparemment local, avec un curieux ⊓ pour Ṣ, ce qui donne les noms: Ṣʿd ṢRMM" (pp. 218-219).

Pirenne's preposterous presentation of the whole group is another example, if at all needed, of her incompetence to deal with rock inscriptions because she steadfastly considers the monumental inscriptions only as a matter worthy of any seri-

ous interest and treats the rest as unworthy of any attention. Therefore, her solution is as simplistic as can be:

she calls "très ancienne écriture sabéenne" the group a - e;
she calls "deux ou trois graffites 'thamoudéens'" the group f - h, and her expression "deux ou trois" shows that either she is unable to distinguish them and to know whether there are two or three, or she does not care to identify them properly;
she calls "d'époque monumentale, d'un style apparemment local, avec un curieux ⊓ pour Ṣ" the text i; and
she simply ignores texts j - l.

Here follow my tracings of those texts:



In fact, only texts f - h really belong to the cursive alphabet; and all the others represent a cursive rendering of the monumental alphabet. Obviously enough, Pirenne's expression "très ancienne écriture sabéenne" means, in her terminology, pre-historical period!

a: the upper line of the left section: ʿm/ḥnʾ ʿAmm [of the family] Ḥâniʾ. - ḥnʾ is a Min personal name in RÉS 3570/1.

b: below and to the right of the preceding text: ḥym 1 Ḥâyim [of the family]
ʾḥr 2 ʾAḥarr.

ḥym, e.g., CIH 358/1. — ʾḥr, Ja 2544b.

c: below and to the left of the preceding text: snrn bn ʾlṯm Sanrân, son of ʾAlṯam. — snrn, cf. Saf snr in CIS 1735. — ʾlṯm, cf. Saf lṯmt, e.g., in CIS 1501.

d: below and to the left of the preceding text: t³rn ḥgg Ta³rân [of the family] Ḥaggag. — t³rn, cf., e.g., Ḥaḍr Ja 923/5, and Sab RÉS 3880/1. — ḥgg, e.g., Ja 2260a.

e: below and to the right of the preceding text: ³ls^{c}d/... ³Ilsa^{c}ad ...

f: to the right of text a, and the upper line of the right section: db³m/ddbš Daba³um, he of (the family) Dâbiš. — db³m, Ja 2491 h/1; cf. also Ar db³ "espèce de sauterelle" (cf. DoSDA, I, p. 483B). — dbš, Sa 126.

g: below the end of the preceding text: ← tlg 1 Gâlit-
→ ³b 2 ³ab.

Cf. the Liḥ personal name gltqs in JaL 144/8.

h: a vertical name to the right of the preceding name and below the center of text f: ↓ mty³ Mutay³. — Cf. Ar mata³a "to beat (a person with a staff or stick)" (cf. LaAEL, p. 2688A).

i: to the right of the preceding text, and below and to the right of text f; PiPH

1. ṣ^{c} 1 Ṣaw^{c} [of the family]
ṣrmm 2 Ṣarmum.

ṣ^{c}: the letter "D" of Pirenne's "Ṣ^{c}D" does not exist; it is the combination of several lines in the rock that gave her the idea of the letter. Cf. Tham, e.g., Ḥâ³il 139a (cf. JaMAR, XIII, p. 169) and Saf WH 1716 (cf. below, VII D nos. 6 and 11, b).

j: to the right of the interlinear space of the preceding text: nt$_{1}$y$_{1}$d Nuta(y)d. The restoration of y is most likely because the nqtl nominal derivation is rare. Cf. Ar natida "quievit, constitit" (cf. FrLAL, IV, p. 238B).

k: below texts i and j: ḥdr ḥbl Ḥadir [of the family] Ḥâbil. — ḥdr, Tham HaLi 276. — ḥbl, cf. the personal name in Liḥ JaL 22a, Saf WH 970+971, and Tham JaT 43/2.

l: to the left of the preceding text and below texts g and h: mlhs Malhas. — Cf. the Saf personal name lhs in CIS 5047.

Ja 3172: further up in the same "wadi Ganadila," another inscription. - PiPH 2.

Pirenne's facsimile: [illegible]

Pirenne's translation and commentary read as follows: "'endroit réservé (et interdit aux autres) par .Y^{c}N du clan de ³Abṣadiq'. (cf. l'arabe ḥagara)."

Pirenne's interpretation of ḥgrm is manifestly disproved by the mimation. — The text may be read as follows: ḥgrm/d^{r}y^{1}^{c}n/d³bṣdq
Ḥagarum Day^{c}ân, he of (the family) ³Abṣadiq.

ḥgrm, e.g., Qat Ja 239. — d^{r}y^{1}^{c}n: the personal name dy^{c} is known in the Qat graffiti from the country of Mukéraš. There is nothing in Pirenne's facsimile that might indicate that a letter is missing between d and y. The space between the two letters in her facsimile most probably indicates a bad spot in the rock the lower edge of which Pirenne took as the part of a letter which could only be d.

Ja 3173: "en haut, à droite" (according to Pirenne, p. 219) of the preceding text, PiPH 3, a short rock inscription written in very ordinary letters found in graffiti, but which is, according to Pirenne, composed of "de petites lettres bizarres, probablement prémonumentales" (p. 219). The only thing that is "bizarre" to the utmost is her insolence in publishing her incompetent description of the letters. Her facsimile is reproduced here: [illegible]. Pirenne does neither transliterate the letters nor give any interpretation of the text which may be read as follows: d³b 1 Da³ab,
(d)^{c}šr 2 (he of) [the family] ^{c}Ašar.

d³b, e.g., Min RÉS 2773/9. — ^{c}šr, also a proper name, e.g., in RÉS 3945/4.

Ja 3174: after mentioning "du 'thamoudéen' vertical et horizontal, du sabéen ancien" (p. 220) of which she does not give a single example, Pirenne publishes another text which she copied "à part, plus à droite," PiPH 4, and, here again, her gross incompetence is illustrated by her comment: "le second et quatrième signes sont de lecture problématique et montrent encore une graphie locale" (p. 220). Pirenne's facsimile (p. 220) is reproduced here: [illegible] As could have been expected, Pirenne does neither decipher nor interpret the text, which may be read as follows:

ẏġr dḥkyl Yaġîr, he of (the family) Ḥukayl.

ẏġr, a family name in Tham JsaT 538. — ḥkyl, cf. the Sab personal name ḥkln in Ja 2187/2.

Ja 3175: " sur le roc de droite, entouré d'épineux, sont gravées sept lignes de caractères anciens, difficilement lisibles dans leur ensemble. Ce sont des noms constituant 5 inscriptions différentes. (On voit, dans l'angle inférieur droit de la pl. V, b, les quatre premières lignes et la fin de la cinquième.)
On y reconnaît les mêmes signes que dans l'inscription ci-dessus: [illegible] et [illegible]." (p. 220)

The text of pp. 219-220 leaves no doubt that the preceding texts and these seven lines are found on the same spot but on different boulders. Yet, the caption of pl. Vb states that those seven lines are "au sa³ab Ma³alaga." This name is given as "ša'ab Ma'alaga" (p. 216). In both cases, "'" and "³" are errors for ^{c}, as

is also the case in "w. 'Abadan" (p. 215) instead of "w. ᶜAbadân," and in "ša'b 'aṣābi'" (p. 231) where each "'" must be corrected to ᶜ; cf. also, the case of "Bīr al-'Awga." Furthermore, the two letters singled out by Pirenne are visible in l. 1, and l.2 reads as follows: dhgn him (or, he) of (the family) Higān.Finally, the photograph is not sharp enough to allow the reading of ll. 1,3; the beginning of ll. 1-2 was destroyed by some water running down the film; no complete letter belonging to l. 5 is to be seen as the consequence of the cropping of the photograph; and the same cropping eliminated ll. 6 and 7. Such carelessness in gathering an epigraphical documentation is matched by her treatment of the few surviving texts.

Ja 3176: almost immediately above and to the left of the preceding group of texts in late Sab type letters; PiPH 5 (p. 220) and pl. Vb.

Pirenne's reading of the text is as follows:

" SᶜDM/MQT — Saᶜadum, serviteur attitré[6]
WY/ŠRHBʾL — de Šarahbi'il
DHBM — Dabhum

Le nom de ce prince n'est pas encore connu." (p. 220)

In her note 6 (pp. 251-252), Pirenne endorses the etymology of mqtwy from the Ar root qtw without answering the objections formulated by JaCMYE (p. 127) against such an etymology, and she translates the word as "serviteur attitré" on the basis of "les lexicographes arabes 'famulus regum' de QTW 'bene famulatus fuit et servivit regibus et proceribus'."

These two quotations are copied from FrLAL, III, p. 399A, with the correction that "famulus regum" is an invention instead of "famulus," which is the translation of Ar maqtawīy. Furthermore, the phrase "famulus ... proceribus" contradicts Pirenne's commentary on "le nom de ce prince." Obviously, a procer = chief will not have a "prince" as servant; and it is a phantasy to represent as a "prince" any person having a mqtwy especially when that person is not even mentioned in the texts as being a chief or chieftain; cf., e.g.,

Ja 690/1-3: an individual is mqtwy of two persons who are said to be bny/zbnr;

Ja 700/1-2: two individuals are mqtwyy of a person who is said to be bn/mqrm;

Ja 708/1-4: an individual is mqtwy of both a person and his son who are said to be bn/hmdn/wdfyš/wsʾrn;

Ja 713/1-2: two individuals are mqtwyy of the two above-mentioned persons who are said to be bn/hmdn; and

RÉS 4420: one individual is mqtwy of a person who is said to be dahr; etc. Moreover, what is the actual meaning of "serviteur attitré"? Whatever meaning Pirenne gives to that expression, the adjective "attitré" is not to be derived from the two Latin quotations she reproduces because they justify only the translation of mqtwy as "servant." Finally, the šrḥbʾl of the present text is presented as an ordinary person in the same manner that none of the Belgian persons bearing the name "Baudouin" is identifiable as the king himself. Although transliterating l.2 as "DḤBM," Pirenne translates it as "Dabḥum," thus reversing the order of the two central letters...Of course, dreaming of a "prince," Pirenne thought that his name should have an appellation, and since the root "dḥb" does not exist in Ar, the metathesis of the two central letters provided her with a name derived from the well-known root dbḥ. However, Pirenne's statement on the author of the text being a "prince" should not after all be taken seriously because she, herself, declares in her following publication (cf. PiDPH, p. 234) that the author of the text was "un officier." It takes a lot of imagination to consider "prince" and "officier" (whether military or civilian) as two synonyms. The same reference gives the reader yet two other equivalent transliterations of her above-mentioned "saʾab Maʾalaga" and of her "Ganadila," viz. "šeᶜeb Maʾlaja" and "Janadila," respectively.

The text may be read as follows:

sᶜdm/mqt	1	Saᶜadum, the strong
wy/šrḥbʾl	2	man of Šarahbiʾil,
dḥbm	3	him of (the clan) Ḥabbum.

dḥbm, cf. Qat AM 60.816/1-2.

Ja 3177: a small funerary proscynema of which Pirenne does not give any detail on the kind of stone (most probably limestone) or the measurements; PiPH 6; my tracing is reproduced on the right. The proscynema originates, according to its owner, from "Qarn al-Karib, au wādī Khaura" (pl. VIa). Qarn al-Ḥarib lies about 44 klm (as the crow flies) east-southeast of Niṣâb according to the map on p. 215. It is true, as Pirenne states, that this kind of proscynema remains otherwise unattested. However, the manner the eyes are represented may be paralleled by that of the fake Hamburg 31.100.1574 (cf. C. Rathjens, Sabaeica, II: Die unlokalisierten Funde, Hamburg, 1955, p. 223, top), and this fake could be an imitation of an original, such as the present one. Furthermore, the

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style of the overall representation does not seem to be rougher or more primitive than that of RÉS 4682 (cf. C. Rathjens, l.c., p. 247, top: Hamburg 31.300.1647). Pirenne's final position is that "cette pièce reste sujette à caution tant qu'une autre similaire n'aura pas été trouvée in situ."

Nonetheless, there is nothing in the text that prevents it from being accepted as genuine: the letter type is as rough as in many cursive inscriptions. Finally, Pirenne considers the second name as "étrange." The basis for such a statement most probably is that it is unattested in SA. But, this could surely not be a valid reason; besides, the name ʿlʾ is in the same condition. — The text may be read as follows: ← hlʾ/lʾdyz 1 Zaydʾil ʾal-
→ ʿ/dʿlʾ 2 haʿ, he of (the family) ʿAlâʾ.

Same reading of the letters as Pirenne's. — ʾlhʿ, cf. Ar lahiʿa "laxus, flaccidus fuit de corpore" (cf. FrLAL, IV, p. 95B). — ʿlʾ is a personal name in Liḥ JaL 131.

PhPH 7 is an upside-down fragmentary inscribed stone (most probably limestone) with hgn "où l'on peut voir HQNY 'a dédié'" (p. 229, and photograph on pl. VIIIa).

Ja 3178 - 3180 are three samples of rock inscriptions (p. 231) on "de petits îlots de shiste" "au wadi Hamman (ou al-Ḥizma)" (p. 230), viz. about 45 klm, as the crow flies, west-northwest of Niṣâb. Pirenne's sole information on all those rock inscriptions is that "on observe des lettres particulières, mais non 'thamoudéennes'" (p. 231).

Pirenne's facsimiles of them are reproduced below, but she does not give her transliteration or her translation of any of them.

Ja 3178 = PiPH 8 (p. 231, left):

The text may be read as follows: sʿdb kmw Saʿdab [of the family] Kamaw. sʿdb, cf. the Sab personnel name yhʿdb, e.g., in RÉS 4405/1. — kmw, cf. the Saf personal name kmwt in WH 707.

Ja 3179 = PiPH 9 (p. 231, center):

The text may be read as follows: kʿtm 1 Kaʿatum [of the family]
yšhrh 2 Yašharuh.

kʿtm, cf. the Saf personal name kʿt, e.g., in CIS 4522. — yšhrh, cf., e.g., the Sab personal name yšhr in Ja 28041.

Ja 3180: PiPH 10 (p. 231, right): a vertical line to be read from top to bottom.

The text may be read as follows: ʿr gnʿ ʿArr [of the family] Ṣanaʿ.

ʿr, cf. Qat Ja 408 d/2. — gnʿ: well-known name; e.g., of a person in Sab RÉS 4623 B/1, of a city in Qat RÉS 3958/8, and of Sab house in RÉS 4702/2.

Ja 3181: a block of sandstone most probably "provenant de Hajar am-Naʾb, réutilisée par une maison de Hajar as-Saʾada-Markha" (pl. VIc); PiPH 11. My tracing is reproduced here. - Pirenne reads the text as follows: " { ḤḍS R M / [. . .
[/] D R Y D N " (p. 238).

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Here again the name "Hajar am-Naʾb" is mistreated: it becomes "Hajar am-Nab" (p. 215) and "Hagar am-Naʾb" (p.238). Also the name mentioned last becomes "Hajar as-S'ada" in the map (p. 215); the town is located about 10 klm, as the crow flies, southwest of W. Hamman. Pirenne does neither explain the value of the brace to the left of her transliteration nor justify it. It is valueless; it cannot mean that the text is composed of two lines because she did not use it in both her PiPH 5 = Ja 3176, and PiPH 6 = Ja 3177. Furthermore, her transliteration suggests that the present fragment is the continuation of a text apparently engraved on another stone. But, the margin on the right is much too wide to support such an opinion. Moreover, the bottom of the first two letters of l.1 is at the same level; that of the others of the same line is higher up. The top of the first letter of l.1 is at a level lower than that of letter no 2, as if the original upper right corner of the stone had already been broken off; and the top of letters nos. 2, 3 and 5 are at the same level, but that of letter no. 4 is at a higher level, viz. the height of each letter is different from that of all the others; the letter height of l.2 is almost 2/3 of that of l.1. Finally, Pirenne's decipherment of the fragment is another proof - if at all needed - of her incompetence: her first word divider of l.1 is y; to the left of the top of the only word divider is what appears to be the right half of y and the break of the stone appears to have followed the staff of the letter y; her "ṣ̊" of l.2 is a symbol; her restoration of a word divider before drydn is unjustified; and she eliminates not only the word divider after drydn, but also the mention of a design to the left of that word divider, which looks very much like a wide r.

The text may be read as follows:

ʿḥysrm/yʾ[... 1 Ḥaysarum Yu[...
Symbol drydn/Design' 2 Symbol. he of (the tribe) Raydân. Design.

ḫysrm, cf. the Qat family name ḫarm in the graffiti from the country of Mukéràs.

PiPH 12: the caption of pl. VIIIb reads as follows:"Village de al-Darb: pierres de construction et inscription WD-ʾB réutilisées dand une maison, provenant de Khazinet ad-Darb."

No mention is made of this photograph on pp. 243-244 in her description of "Khazinet el-Darb." The three variants of the same name in the two quotations, and even two variants in an extremely short caption are an invitation to conclude this paper with yet other examples of Pirenne's inability of holding on to one single transliteration of a name (cf. already above, e.g., p. 20):

	PiPH	PiDPH
al-Darb	pl. VIIIb	
ed-Darb	p. 215	p.206
ad-Darb	p. 249	
el-Darb	p. 243	
Khazina	p. 215	
Khazinet al-Darb	p. 246	
Khazinet ed-Darb	p. 249	
Khazinet el-Darb	p. 243	
Khazinet ad-Darb	pl.VIIIb, IXa, XIVb	
al-Khazina (Khazinet ed-Darb)		p.206

III - Notes on the texts from the country of Raḥab (PDRY) published by J. Pirenne.

A - Introduction.

In a newspaper-style account of her two trips into the country of Raḥab (northeast of Mukéràs), PiDPH, J. Pirenne deals with 24 texts in a careless and incompetent manner wholly dominated by her reckless imagination which makes her not only take her dreamy concoctions for the reality but also bends and distorts the facts to suit her theories or preconcieved ideas.

The real test of an epigrapher is not at all to read well-carved inscriptions, but to decipher battered texts. Furthermore, her publication of rock inscriptions is selective. As usual, she chooses what she likes or finds easy for her overall interests. Although stating that there are "de nombreuses inscriptions" (p. 215), only two of them are shown on a photograph of pl. VIIc, viz. PiDPH 6 and 7, and the others, shown on the facsimiles of fig. 2 (p. 217), were selected because, as she puts it, they are "celles qui sont le mieux visibles" (p. 215). It would be difficult to show more clearly that those precious human relics are held in such small an esteem that they do not deserve any special effort. Pirenne's lack of deference is such that she does not even care to mention how many inscribed boulders there are and/or which of those rock inscriptions are on the same boulder or on any side of a boulder.

B - General remarks.

1 - Pirenne is not aware that her two Qat texts PiDPH 20 and 23 had already been published in 1971 and 1972, respectively.

2 - Pirenne gives no interpretation of PiDPH 6-8 and 10-17.

3 - Beside her gross misreading in PiDPH 23/2, center, Pirenne also misreads two other letters in the rock inscriptions, viz. her "L" of PiDPH 7/1, and her "Š" of PiDPH 12, instead of g and r, respectively. For the confusion of g and l, cf. JaMAR, XIV, pp. 75-78.

4 - The gender is very often mistreated; in all the cases, but one, the grammatical idiomatic characteristic of the texts is misrepresented and distorted.

Pronouns. - The singular -s is translated as a plural, "leur," in PiDPH 3/2, 3;

the plural -sm is translated as a singular, "ses," in PiDPH 21/7, 22/8, and "son" in PiDPH 22/2, 7.

Verb. - The singular s^{c}šq is translated as a plural, "ont fait," in PiDPH 3/2.

Divine name. - The singular mnḍḥ is translated as a plural, "divinités protectrices," in PiDPH 5/4.

5 - The relative pronoun d is mistreated in two different ways: it is either simply eliminated from the translation or erroneously understood as a part of the name it introduces. The expression d^{c}dbtm, where d is the relative pronoun introducing the well-known temple name ^{c}dbtm, is rendered either as "de ^{c}Adabatum" (PiDPH 3/4-5; thus d is not translated) or "de D^{c}BTM" (PiDPH 5/5; beside the omission of d of the proper name, the relative d becomes a part of it). The handling of "DRYMN" (PiDPH 21/8 and 22/7) is the same as the first seen in the case of d^{c}dbtm, viz. the relative pronoun is not translated. Note also the difference between "de Raymān" in PiDPH 21/8 and "de Rayman" in PiDPH 22/7. In the expression š^{c}bs/ḏḏr^{ɔ} (PiDPH 21/6) and š^{c}bn/ḏḏr^{ɔ} (PiDPH 22/8), d is treated as in the second case of d^{c}dbtm, viz. the pronoun is repeatedly mistaken as a part of the tribe name, and becomes "du-" or "Du-," respectively. Furthermore, š^{c}b is erroneously translated as "communauté;" it means "tribe."

The feminine relative pronoun dt in some names of the solar goddess is rendered either "Dāt" (e.g., PiDPH 21/7) or "Dat" (e.g., PiDPH 21/7-8).

The divine name dt/ḥmym is particularly mistreated: "Dāt-Ḥamīm" (PiDPH 21/9), but "Dat-Ḥamim" (PiDPH 22/7).

The most mistreated name, however, is that of ^{c}Amm's temple rḍḥm. Correctly transliterated in PiDPH 21/8, 9 and 9-10, it becomes "RDḤM" in PiDPH 22/7. In all places the translation has "Radiḥum."

6 - The best example of Pirenne's aberrational way of argumenting is her note 23 (p. 238), which reads as follows:

> " 23) ĠBR cf. RES 3879 "prolétaires" selon N. Rhodokanakis, ce qui est anachronique. Dans le contexte sud-arabe, ce serait soit des esclaves, soit simplement les "hommes valides" ; cf. LANE, *Lexicon*, p. 374), mais RES 3879 citant les femmes et les hommes (ʾDWM) ce seraient plutôt les esclaves."

It would not have been such an effort to give the complete reference to RÉS 3879/3. — On the personal substantive ġbr, it should be noted that, in RÉS's commentary (V, p. 325), the first letter of the second Ar word quoted should be corrected from "^{c}" to ġ; the last sentence of RÉS's paragraph concerns ġbr, not wmy; and the last pericope, "en relation avec ġbr," must be suppressed. — Pirenne states: "'prolétaires' selon N. Rhodokanakis, ce qui est anachronique." But, she fails to justify how it is or could be "anachronique" as if the social reality described by the Ar expression banū l-ġabrāʾ "the poor, needy, or indigent" (cf. LaAEL, p. 2224C), quoted by Rhodokanakis was not as old as the world itself, and as if the word "prolétaires" had not been coined before the birth of both K. Marx and F. Engels. — The pearl of Pirenne's note is "les 'hommes valides'; cf. LANE, Lexicon, p. 374):" in order to interpret Qat ġbr (also in PiDPH 21/6), she invokes the Ar root غبر which is presented in LaAEL, pp. 373-375B, instead of غبر which is to be found in l.c., pp. 2223A-2224C. Furthermore, not a single word of LaAEL, pp. 373B-375B justifies her suggestion of "hommes libres," not even jabbār "one who magnifies himself, or behaves proudly or haughtily or insolently, and does not hold any one to have any claim upon him, or to deserve anything of him; one who slays when in anger; etc..." (pp. 374C-375C). Even a slave can have those feelings regardless of his condition. — Pirenne also confuses the two words ʾd(w/y)m(t) and ʾ(n)s when she translates the first as "hommes." It has been known for a long time already that the first serves as the plural of ^{c}bd servant. — Finally, the context of RÉS 3879/3 does not support Pirenne's deduction that "ce seraient plutôt des esclaves." The enumeration of RÉS 3879/3 is

...s]m/wʾmtysm/wʾdwmsm/wġbrsm/wwmysm/ -

... the]ir ... and their maidservants and their manservants and their poor and their sworn persons ...

Rhodokanakis assumes that y of wmy belongs to the radical of the noun; this etymology, however, must remain doubtful; cf. ʾmty. If Rhodokanakis's reference to Syriac yimi is justified, the substantive could be translated as "the sworn persons." As the social organization of the SA family unit still remains largely unknown, it is preferable to remain vague instead of inserting into the words unsubstantiated ideas. However, the presence of ġbr and wm(y) side by side may suggest that both words refer to two sections of destitute peoples, the first remaining free from any allegiance to any particular family, and the second having taken some oath of allegiance toward the family supporting them.

7 - The tracings of PiDPH 6 and 7 are mine; those of PiDPH 8-17 are those of Pirenne reproduced in fig. 2 (p. 217).

C - PiDPH 1 is described on p. 207 as "un pot en pierre, couvert d'un enduit brun;" cf. photograph on pl.IIIc, d and e. — Pirenne reads "deux noms propres GWRN et ^{c}KBT." — The inscription may be translated as follows:

^{c}Akbat [of the family] Gawrān.

ʿkbt is a feminine personal name in Qat TC 3002. - gwrn, cf. the name of Qat tomb in Ja 331/2. ʿAkbat was the owner of the pot.

D - PiDPH 2 (p. 213, and photograph on pl. VIIIb). — Pirenne claims that it "est malheureusement illisible à cause de l'éclairage et de la surcharge d'un graffite arabe." Her photograph is indeed practically useless, and it is difficult to figure out where the Ar graffito might be or to see where "p" which she reads is located. It looks very much like that Pirenne did not even try to read the text from the stone itself although it was all the more necessary that the lighting was not very favorable, and she limited herself to taking a photograph.

E - Ja 3182 = PiDPH 3.

Introductory note on PiDPH 3 and 4 (pp. 213-214, and photograph on pl. VIIa).

The photograph is printed upside down. — Pirenne fails to point out three features of the two texts, viz. the interlinear space is filled with a horizontal line; a vertical line runs alongside the beginning of PiDPH 3; and a serrated line fills the space between the two vertical lines located between the two texts; these two lines are reproduced in Pirenne's transliteration of the inscriptions.

Ja 3182 may be read as follows:

1 lḫymm/dyʿlf/wd²bm/šm
2 sʿšq/wshḍt/byts/yḥḍn/b
3 nšrsm/ʿdfrʿm/brd²/mr²s/yš
4 ...]ṯsn/ḏʿṯm/dḏʿṯd
5 [btm/...]

1 Luḥaymum, he of (the clan) Yaʿlaf, and Da²abum, has assured
2 the working on, and the restoration of, his house Yaḥaḍḍân
3 from bottom to top with the help of his lord Yaš-
4 ...].. of ʿAmm, Him of ʿAd-
5 [batum ...]

L.1: the letters of this line are wider than those of the other lines. - The initial letter is l, instead of Pirenne's "Ĉ;" she did not recognize the vertical line to the right of the text.

Ll. 1-2: according to Pirenne's interpretation, the secondary author would have a double name, viz. "D²BM ŠM," while the primary author would have a single name only. Such a feature is unacceptable. — d²bm is the name of lḫymm's wife. — šm is the well-known defective form of šym, which governs two maqdar's, sʿšq and shḍt. The verb šm is in the singular as lḫymm is the principal subject and responsible of the whole work. The verb shḍt means to renovate, renew; Pirenne's translation of the verb as "inaugurer" is incorrect (cf. the commentary on Ja 2977/2). — The remodeling of the house was quite substantial; it affected the whole house from bottom to top according to ll.2-3.

L.5: the brackets were forgotten in Pirenne's transliteration, and three dots must be added after the word divider. These two remarks are also valid for the translation.

F - PiDPH 5 (pp. 214-215, and photograph on pl. VIIb). — The photograph is very poor.

L.1: instead of Pirenne's reading of ".....] ²LT/W.NSWW/.B[...," read ...]rbn/d²lt/wbnsww/sb[... ...]rabân Da²ilat and his son Šab[...

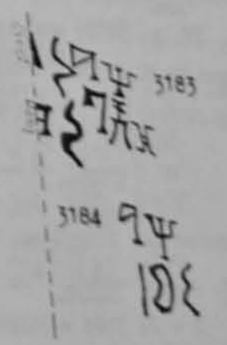
L.2: instead of "Tʿ" at the beginning, read ntʿ. — Pirenne's restoration of "B[..." as "B[YTSM]" is erroneous for the obvious reason, which Pirenne apparently did not see, that l.2 would be much shorter than all the other lines.

L.3: there is a vertical line before yfʿn, which is not registered in Pirenne's transliteration; and that line suggests that there was a vertical line flanking the right edge of the text. — "leurs" before "seigneurs" in Pirenne's translation must be put between brackets.

L.4: Pirenne's translation of mndh as "divinités protectrices" is erroneous because the word is a singular (cf. already above, p. 30); cf. JaMAR, XIV, pp. 271-273. — In Pirenne's translation "leur communauté," the personal pronoun must be put between brackets. Furthermore, it should be repeated here that šʿb means "tribe," not "communauté" (cf. already above, p. 30). — Finally, "HD²" restored at the end of the line has no counterpart in her translation.

L.5: to the right of ʿm, there is the left part of w and, which is not to be found in Pirenne's transliteration.

G - Ja 3183 = PiDPH 6 (p. 215, and photograph on pl. VIIc): ḫgrm 1 Ḫagarum,
d²ǧrb 2 he of (the family) ²Aǧrab.



Pirenne's treatment of this short text exemplifies her carelessness; her transliteration contains no less than four errors: (1) her "H" instead of ḫ, although her reference is to HaAIC, "p. 177," which lists ḫgr; (2) her "Ĉ" instead of ǧ, although the letter is perfectly clear on the

photograph (her "Ḏ" is also used to transliterate ġ in PiDPH 21/6, 22/8 and 23/5); (3) the omission of the vertical staff of m at the end of l.1 still clearly visible on the photograph; and (4) her "D" instead of d. The type of ġ of the text is related to no. 10 of my summary presentation in JaMAR, XIV, p. 300. — The edge of the photograph cut off the left half of the last letter of each line.

H - Ja 3184 = PiDPH 7 (p. 215, and photograph on pl. VIIc).

Pirenne reads the nonexistent "W" between the preceding text and the following name: ḥg 1 Ḥaga-
rm/ 2 rum.

Pirenne's study of this very short text is another example of her carelessness: she erroneously reads "ḤL
ḤM;" omits the word divider; and refers to HaAIC, "p.197: ḤRLM" where the correct name is ḥlrm. Since she read "ḤRLM," it is certain that she had that name in mind and that, in turn, suggests that she read the text as two vertical lines to be read from top to bottom and composed of two letters each; viz. ġ ḫ 3 1
/m r 4 2.

I - Ja 3185 = PiDPH 8 (p. 215, and Pirenne's facsimile in fig. 2, 1 on p. 217).

Pirenne's treatment of this text is another example of her incompetence: she reads the inscription as follows: "MʿŠR
ḤTM
ʾḤW." 1

Since she does not care to give any interpretation of the text, nobody knows why she deliberately dissociates her "ʾḤW" from l.1 and makes her reader believe that it stands not only alone but also below and to the right of l.2. Furthermore, nobody knows how she justifies three personal names next to each other. Moreover, she refers her "ḤTM" to HaAIC, "p. 608 : ḤT." Her etymology is erroneous, since the root ḥtm does exist (cf. Ar ḥaṭama "contudit subtiliter rem in pulverem" in FrLAL, IV, p. 367B) and is attested in the Tham personal name ḥtm in Dghty 51,2, and ḥtmn in Hu 729/2. Finally, here again, Pirenne eliminates the word divider at the end of her l.2. - The text may be read as follows:

mʿšr ʾḫ w 1 Muʿâšir, brother of Wa-
ḥtm/ 2 ḥtum.

The engraver had forgotten the letter r which he added after engraving at least ʾ. For whtm, cf. Ar wahaṭa "to be persevering, or assiduous" (cf. LaAEL, p. 2970A).

J - Ja 3186 = PiDPH 9 (pp. 216, 218, 234, and facsimile in fig. 2,3 on p. 217).

Pirenne does not explain the reason why the only line of her translation, viz. "Qu'écoute le Seigneur du ciel." on p. 218 is separated from the transliteration of the text on p. 216 by the three lines of PiDPH 10 on p. 217. — The text may be read as follows: lysmʿn/bʿl/smy 1 May the lord of heaven lis-
nʿ/ nt /wm(d)y 2 ten to ʿAnat and Madi.

L.1, cf. Pirenne. — L.2: Pirenne only retains the beginning "N/" which is followed by ten points. No reason is given for the number of dots although her own facsimile makes it certain that there cannot possibly be ten letters missing. Furthermore, these dots contradict the period after "ciel" of her translation. As Pirenne's facsimile does not show any hatching between the first two letters of ʿnt and/or between ʿnt and the following word divider, the reader must assume that there is no letter missing in her facsimile. Finally, the kidney-shape design at the end of the line which is below the horizontal axis of the letters, must represent some feature in the rock.

K - Ja 3187 = PiDPH 10 (p. 217, and facsimile in fig. 2,4 of the same page).

Pirenne's reading is correct, but she fails to register the word divider at the end of each line.

sdt/ 1 Sâdit,
ʿbn/ʾsd/ 2 son of ʾAsad.

sdt: Pirenne perpetuates a reading error of the first copyist, H.St.J.B. Philby (cf. Le Muséon, 51 [1938], no. 57) by referring to HaAIC, "p. 314 : SDṮM," where the name is said to be in Ḥaḍr RÉS 4884/1-2. HaAIC's remark on the name includes a contradiction, viz. "Root unknown. Cf. ʾlsdt." The definitive reading of RÉS 4884 was republished as Ja 972 eight full years before the printing of HaAIC in JaUT, p. 55 and tracing on pl. 4. The name in Ja 972/1-2 is kdtm.

L - Ja 3188 = PiDPH 11 (p. 216, and facsimile in fig. 2,5 on p. 217).

The six lines of the text appear to have been engraved by the same person; the beginning of the lines is well aligned. Although the text apparently is incomplete, there is no hatching to the left of what Pirenne reads of the ll. 1, 2, 3, 5 and 6; and the reader is not informed whether or not there are traces of letters below l.6. Finally, Pirenne's reading does not register /hw of l.4 and the whole of l.6. — The text may be read as follows:

....n/ḥmln/[d]	1	[....]ân Ḥamlân, [he of (the family)]
mrṯd²(wm)/q(d)[m]	2	Marṯad²a(wwâm), com(man)[der of]
dqhlm/ʿmn/. ...	3	the tribe Qahilum, has dwelled ...
w./hw.	4	...
ḏš(r).(m)/[...](s)ṯy	5	him of (the family) Ša().(rum) ... sixty .
wḫmr/bḏtm	6	...

Pirenne's translation is marred with two errors, viz. her "ḪMLN" (l.1; cf. also p. 234) instead of ḥmln (cf. also her reference to HaAIC), and her "²TY" (l.5) instead of ²ṯy (cf. also her reference to HaAIC). — Pirenne isolates four names out of the six words of the transliteration. — For qdm/ḏ, cf. Ja 681/3. — The second half of l.3 and ll. 4 and 5 are so fragmentary that any attempt at interpreting l.6 is impossible.

M - Ja 3189 = PiDPH 12 (p. 216, and facsimile in fig. 2,6a on p. 217).

²b²rb ²Ab²arab.

Pirenne misreads the fourth letter as "Š." The name is known in the Qat graffiti from the country of Mukérâs.

N - Ja 3190 = PiPDPH 13 (p. 216, and facsimile in fig. 2,6b on p. 217).

kml (as in the edition) Kâmil.

O - Ja 3191 = PiDPH 14 (p. 216, and facsimile in fig. 2,7 on p. 217).

mtʿ[m]	1	Mataʿum
ytmm	2	Yatamm,
ḏ²yrb	3	he of (the family) ²Ayrab.

For the transliteration, less the two half-brackets, cf. the edition. - ytmm: Pirenne refers to HaAIC, "p. 657" but fails to point out that HaAIC lists ytm only, not ytmm. — ²yrb is a qytl nominal derivation.

P - Ja 3192 = PiDPH 15 (p. 216, and facsimile on fig. 2,9a on p.217).

ʿrb (as in the edition) ʿArab.

Q - Ja 3193 = PiDPH 16 (p. 216, and facsimile in fig. 2,9b on p.217): to the right of the preceding name and in letters twice as large according to the facsimile. ʿšz ʿAšîz. - Cf. the family name ʿšzm in RÉS 4440. — Pirenne states that the name is "à lire en se plaçant de l'autre côté de la pierre;" and she reads it as "ZRʿ." Why would the engraver ... have crawled to the top of one side of "la pierre" to engrave his name on the other? The objection is all the more serious in that Pirenne herself states that the inscriptions were engraved on "de gros rochers durs" (p. 215); apparently one of those boulders became "la pierre" for the sake of her theory. — Pirenne refers to HaAIC, "p. 297," who lists only one Tham text, WiAM T13; cf. AION, 31 [1971], p. 445 and facsimile on fig.1: The first five letters contain two pairs of two identical letters, viz. nos. 1 and 2, and nos. 3 and 5. F.V. Winnett reads the first group as "l" and "(z);" and the second as "r" and "b," respectively... without justifying the discrepancies. The initial name is lbʿ Lâbiʿ. Winnett also claims to find zrʿ in Liḫ JsaL 298. In this text, which was republished as JaL 130a, zrʿ is the verb has sowed (cf. JaMAR, VII, p. 87).

R - Ja 3194 = PiDPH 17 (p. 216, and facsimile in fig. 2,9c on p. 217). According to the facsimile, the text is located below and to the right of the two preceding names.

ʿšbm/yšb ʿAšbum [of the family] Yašab.

For the transliteration, cf. the edition. — Pirenne's facsimile does not make it clear whether the lines above and below the text represent the edges of the rock or belong to an incomplete cartouche. — ʿšbm, cf. Min ʿšb in Ja 2250b.

S - Ja 3195 = PiDPH 19 (pp. 220-221, and photograph on pl. Xc). Above the six lines of her transliteration and translation, Pirenne adds a line composed of seven dots; she thus must consider the upper line of the text as missing. Yet, no traces of letters can be detected there. — The six lines of the text are read as follows by Pirenne:

" HW[F]ʿT[T/	30 dots	/M]	37 letters
ḤFDS/.ʿ[	32 dots	/B]	39 letters,
N/ŠRSM/ʿD/FRʿM/W	25 dots	]	37 letters;
./ʿM/W²NBY/[	30 dots	]	38 letters;
RM/BʿM/WB/²N[BY	27 dots	]	38 letters;
K.W/[	37 dots	]"	40 letters.

Pirenne's long series of dots in each line remain without any justification since it is not based on a single complete line, original or restored, viz. there is no reason why she could not have shortened or lengthened her lines by 2, 3 or more units. It also seems certain that, once she had assigned 37 letters to l.1, the number of dots to be given to each subsequent line was demanded by her desire of presenting her transliteration of the whole text as ... a perfect rectangle. As a matter of fact, it would have been nicer to look at had Pirenne added 20 more dots

to each line: her transliteration would then have filled to capacity the whole width of the printed space of the page.
The whole text may be read as follows:

1 hwfᶜt[t]/bn/s[flyn/bny/m]
2 ḫfds/yfᶜn/wṣw[btsww/b]
3 n/ʾrsm/ᶜd/frᶜm/...
4 ./ᶜm/wʾnby/w...
5 rm/bᶜm/wb/ʾn[by/...
6 kh[...

1 Hawfᶜat[at] of (the group) Sa[flîyân, has built]
2 his [t]ower Yafaᶜân and [its] buttr[esses fr]om
3 bottom to top ...
4 . of ᶜAmm and ʾAnbay and ...
5 rum. By ᶜAmm and by ʾAn[bay ...
6 ...

Ll. 1 and 2 are composed of 17 letters each. — sflyn is known as a Qat ethnical group in Ja 311. — To the right of frᶜm/ (1.3), Pirenne reads "W." The remaining design on the stone is angular and cannot be read "W." This remark applies also to Pirenne's "w" in 1.6.

T - Ja 2357 = PiDPH 20 (pp. 221-222 with D.B. Doe's facsimile reproduced in fig. 4 of p. 221).

Pirenne fails to inform her reader that the text was published as Ja 2357 in JaMAR, II (1971), p. 92, the same year as Doe's publication, but independently from Doe as my study was based on a copy made by M. Van Lessen. — My study of the inscription, which is maintained here, is reproduced below:

Ja 2357 : copie de M. Van Lessen (VL C[opy] 21) d'une "stone from Am Adiya at Mukeiras Hotel. Letters 1 1/2 inch high"; extraits des l. 1-2 dans WiZAG (p. 68) d'après un fac-similé de D.B. Doe.

1 ...]cqrbn/lʾlwds/wbyts/yfᶜn/ʾrḏm[...
2 ...]m/bbḏc/hgrn/rḫb/ṭg/ṭrg/wdm/tm[sʾls/...
3 .../ly]zʾn/ṣdq/wsʾmn/cqrbn/bkl/ʾrḥ/tkb[...

1 ...] ᶜAqrabân pour ses enfants et sa maison Yafacân des pierres de bâtisse..
2 ...]. dans la contrée de la ville Râḥib selon l'injonction de Waddum dans [son] or[acle, ...

3 ... qu']Il [conti]nue à justifier et à assurer la sécurité de ᶜAqrabân dans toutes les affaires ...

La pierre porte la partie centrale d'une inscription commémorative de construction, dont les l. 2-3 rappellent AH 736/2-3.
yfᶜn au lieu de yfn de WiZAG. - ʾrḏm est traduit "das Ackerland" par WiZAG. Le "das" est exclu par la mimation. Cf. l'ar raḍm et raḍam (collectif) "pierres de bâtisse, moellons"; le nom d'unité est raḍmat. - rḫb est complet, contrairement au texte de WiZAG. - ḥrg est un substantif, comme dans AH 736/2; HBOWI (p. 455) le prend pour un verbe, ce qui force cet auteur à suppléer un complément direct inexistant, "ihm". - sʾmn, RÉS 4336/4.

Pirenne's translation and commentary.

In 1.2, her "son" must be put between brackets.

In 1.3, her grammatical rendering of the first three words is unjustifiable and belongs to some aerial acrobatic. — The plural substantive ʾʾrḥ is translated as a singular, "affaire" or "une situation" (p. 222, l.9 from bottom) because of her erroneous understanding of ʾʾrḥ as the subject of the singular verbal form tkrb.

Doe's facsimile has tkrb instead of Van Lessen's tkb. Pirenne's treatment of tkrb reads as follows:

" TKRB. Ce verbe KRB est interprété généralement par "offrir, honorer" qui ne convient pas ici ; nous reconnaissons le sens du verbe, en arabe : "It (sorrow, grief, or affair) afflicted, distressed or oppressed him"(15) "

Her note 15 (p. 238) refers to LaAEL, "p. 2602." Pirenne's treatment of LaAEL's testimony distorts voluntarily the latter's written word, viz.

" 4. أكربهُ [*He*, or *it*, *affected him with* كَرْب, i.e. *sorrow*, *grief*, *distress*, or *affliction*: occurring in the TA in several places.] = اكرب, inf. n. إِكْرَابٌ, *He filled* (K) a skin. (TA.)" and

" 8. اكترب *He became afflicted, distressed, or oppressed, by sorrow, grief, &c.*, (K,) or *by an affair*: (TA:) so also تَكَرَّبَ, aor. ـُ (TA.)" (cf. LaAEL, p. 2602B).

Pirenne's "It" and "him" come from the 4th form;
her "sorrow, grief, or affair" come from the 8th form. In LaAEL, however, these words are modal complements of a passive expression, viz. "become ...;" Pirenne makes them to be the subjects of active verbs;
her "afflicted, distressed or oppressed" also come from the 8th form, and Pirenne presents them as verbs in the active voice. In LaAEL, however, they are parts of expression in the passive voice, viz. "became afflicted, distress-

ed or oppressed."

It is hard to have any respect for the argumenting method of an author who so flagrantly misconstrues and distorts the written word and yet seems to be perfectly at ease and confident in the correctness of her understanding since she herself puts her travesty of quotes between quotation marks with reference to the original. Furthermore, contrary to Pirenne, the verb tkrb (e.g., Ja 2438/3) does not mean "offrir, honorer;" it means to assign, allot.

If Doe's copy is correct, tkrb[cannot be the perfect of the 4th verbal form as the whole phrase of l.3 deals with the future; it has to be a substantive, tkrb or tkrbt(?) assignment. The pericope ʾʾrḫ tkrb[(t?)s may be translated as the affairs of [his] assignment.

U - PIDPH 21 (p. 227, and photograph on pl. XII).

L.2: the expression mkrb/qtbn/wwld/ʿm is translated as "roi de Qatabān et des fils de ʿAm."

Pirenne most probably copied the wordings of this pericope from CIASA, p. 110. This suggestion explains the word "roi" (an error instead of mukarrib) and the strange spelling of "ʿAm." For the translation of that expression, cf. the commentary on Ja 2973 (to be published). Finally, in order to justify her spelling "ʿAm," Pirenne considers the second m of ʿmm as the mimation (cf. CIASA, p. 113). Such an opinion is erroneous because the root is ʿmm.

L.3, translation: the spelling of "premier né" without hyphen is erroneous.

L.4: dʾmr/wšmr is translated as "celui qui commande et qui s'active" and, according to her footnote 21 (p. 238), the relative pronoun d refers to the author of the text, whether a mukarrib or a king. This opinion is unacceptable. The idea of "s'activer," viz. "to work briskly, hurry up," can hardly fit a king or a mukarrib; a verbal pericope introduced by a relative pronoun and inserted in the identity card of an individual is a feature unknown in the inscriptions; but a determinative composed of temple names introduced by d/dt is the rule; cf. already Le Muséon, 60 (1947), pp. 79 and 83, and repeated in BASOR, No. 138, April, 1955, p. 46. To interpret ʾmr/wšmr as verbal forms because they do not have the mimation is to ignore the whole epigraphic material; e.g., dʾwm, e.g., in Ja 735/8; for ʾwm from the root ʾwm, cf. JaMAR, XIV, pp. 107-109 and 133; cf. also yrsm and šqr in the commentary on Ja 2972 (to be published).

Note 21 of Pirenne's text refers to CIASA, p. 112, which reads as follows:

"A. JAMME traduit comme s'appliquant au roi, "Him (sic) of ʾAmar and Šamar', qu'il a explique dans 31.1965.-, p. 46-47."

This short note is another illuminating example of Pirenne's characteristic way of argumenting. Her "31.1965.-" means "A. JAMME, Notes on the Published Inscribed Objects Excavated at Heid bin ʿAqīl in 1950-1951, Washington, 1965 (Mycopié, publication personnelle)" (cf. CIASA, Bibliographie générale systématique, Louvain, 1977, p. 136). Note that the second comment between parentheses is erroneous (cf. already Orientalia, 48 [1979], p. 281). Pirenne's above-quoted note is wholly erroneous. (1) The "(sic)" proves that Pirenne still did not know in early 1980 what has been known for many years already, viz. that I capitalize the initial letter of all pronoun referring to a deity so that the reader would know even at first sight that the pronoun involved does not refer to a human being or an association of human beings. She also is apparently unaware that "dans toutes ses traductions reproduites dans le CIASA, A.P.L. Beeston adopte, sans le dire!, mon initiative d'imprimer en capitale la letter initiale de tout pronom se rapportant à la divinité'" (cf. Orientalia, l.c., p. 265).

As a consequence of her ignorance, Pirenne ascribes to me an opinion which the capitalization of that inital letter excludes explicitely even at first sight.

(2) Pirenne voluntarily misquotes my translation by omitting "(the temples)" after "of."

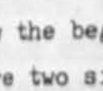
(3) She erroneously transcribes "Šamar" instead of "Šamir."

(4) The justification of my translation of dʾmr/wšmr is not to be found where Pirenne claims it to be. As explicitely mentioned above, the translation is explained in both Le Muséon and BASOR.

The pericope bny/wgnʾ/wṣyr/wshḍt is translated as "a construit et entouré d'une muraille et a achevé et inauguré."

The preceding interpretation of ṣyr as "a achevé" is erroneous because the verb does not, and cannot, mean implicitely that the work was not completed; the verb ṣyr means to dress (cf. the commentary on Ja 3025/2; to be published). Similarly, the verb shḍt does not mean "a inauguré;" for the meaning of the verb as to renovate, renew, cf. the commentary on Ja 2977/2 (to be published).

L.9: Pirenne's transliteration "w[9]B/(WB)/DRḌḤM," the parentheses are given the unusual and unexpected meaning of containing the part of the text which was erroneously engraved by dittography; they should also include the word divider after the second "B."

Below the beginning of the text and to the right of the last, very short l.10, there are two signs, and  , respectively, which become "T" and "Ṯ" = "symboles" in Pirenne's edition. The letter "Ṯ" is incorrect because the lower aslant appendix remains unaccounted for, and the editor fails to point out that these two

symbols indicate the author of the text as a Qat ruler (cf. commentary on Ja 2976, to be published).

V - PiDPH 22 (p.228, and photograph on pl. XIII).

The underexposed published photograph does not make the reading of most of the letters possible.

L.1-2: the restoration of "YD" at the end of l.1 to read the name "[YD]ʿ ʾB" remains unjustified because the following word "BḌR ʾ" cannot be the epitheton "BḌR" of her translation where the final "ʾ" is eliminated, as Pirenne claims it to be; it means in Ḏaraʾ, the place where šʿbn/ḏḏrʾ the tribe of Ḏaraʾ resided.

L.3: the name of the city restored by Pirenne as "ʿBR" should be corrected to "ʿBRN."

W - Ja 2457 = PiDPH 23 (pp. 229-230 and pl. XIV).

Here again, Pirenne fails to inform her reader that the text was published as Ja 2457 in JaMAR, III (1972), pp. 51-52. Here is the text of my publication:

Ja 2457. - A text engraved on a stone found at Ḥuṣn ʾAmm Ṣulibat ḏī Mirrān, cf. the facsimile on pl. 14. - VL C 1.

ʾAmm Ṣulibat is "situated in the Wadi Abadān in the Upper Aulaqi Sultanate, on a hill in the middle of a ruin field and is constructed of ancient stones. Dhi Mirrān is the name given to this area of ruins" (V.L.); see "al-Silabah" about 12 kil. souhwest of Nisab on the map Series K 563, ND-38-7; Edition 6-GSGS.

1 rydʾl/wyšfʾl/nʿmwd/ḏtw/ḏrḥn/šʾmw/wbrʾ/w
2 ẓrb/bythmy/grl/wʾḫṯbs/wṣrḥtsww/wʾmtʿsm
3 wnfshysm/klsm/glm/bḥg/ʾnby/wʾl/tʿly/bʿṯtr
4 /wʿn/wʾnby/wbšmn/yṯʿn/wḥrmn/wbḏt/ṣntm/wb
5 mrʾsm/wrwʾl/ġyln/yhnʿm/wʾḫhsww/ʾmlk/qtbn

1 Raydʾil and Yašafʾil [of the family] Naʿamwadd, the two men of [the clan] Ḏarḥān, have bought and constructed and
2 secured title to the house of them both, Garal, and its workshops and its upper rooms and their guest rooms
3 and their roof terraces, all of them together, according to the law of ʾAnbay and ʾIl Taʿalay. By ʿAṯtar
4 and ʿAmm and ʾAnbay and Bašamum Yaṯaʿān and Ḥarmān and by Ḏāt-Ṣantim and by
5 their lord, Warawʾil Ġaylān Yuhanʿim and his brothers, the kings of Qatabān.

VL C occupies two pages facing each other in the notebook; a number indicates the beginning of the first four lines; the copyist himself corrected /ḏ? in l.2 to ṯ.

VL C contains a few errors which can easily be corrected. The corrections involve data which are so well known that they must be considered as certain; therefore, they are not put between parentheses in the transliteration.

L.1: rydʾl, cf. the well-known rydn and also rydm, a Ḥaḍr personal name in Ja 943. - yšfʾl, e.g. Ḥaḍr Ja 972/1. - nʿmwd, CIH 330/1. - ḏtw is normally a plural; here, as in Ja 119/1, it is a dual.

L.2: bythmw is the only presence of a Sab pronoun in the text. - grl is known as a personal name in HI 2 B (cf. JaIHBH, p. 337 A).

L.3: ʾmtʿsm: RyISA (IX, p. 117) translates the word as "objets mobiliers" on the basis of Ar matāʿ (not mātaʿ) "household goods, utensils and furniture." This interpretation is difficult to accept because, here as in Groom-Beihan 19 a and RÉS 3962 (see below, the appendix), ʾmtʿ is a part in a series of words, each of which indicates a section or building of a house, and all of them depend at least on two verbs, among which is brʾ "to construct." It seems obvious that nobody "constructs" "household goods." I therefore suggest referring mtʿ to Daṯīnah matīʿ "guest" (cf. LaGD, p. 2675) and translating the word as "guest room." - ʾl/tʿly, cf. JaMAR, I, pp. 16-17.

L.4: bšmn, e.g. TC 1335/6 (cf. JaNPIO, p. 54). - yṯʿn is here a divine epithet which most probably is the name of a temple consecrated to the god. - ḥrmn, e.g. RÉS 311/4.

L.5: wrwʾl/ġyln/yhnʿn, e.g. Ja 122/6. - ʾḫhsww, AM 736/9. In this text and here, ʾḫ introduces a plural.

Pirenne's published photograph has no scientific value since she admits that "j'ai repassé les lettres visibles" (p. 230). Furthermore, how can anyone justify or even explain the metamorphosis of "YGRʾL" of her transliteration of l.2 to "YGRṢM" of her translation? How can the editor of a Qat inscription be as incompetent as to release the transliteration of a pericope as full of errors as "WʾḪ/BḎS/WSRḤŠSWW/WʾMTʿN/Ḏ"(l.2) which undeniably is a misreading of wʾḫ ṯ s/wšrḥtsww/wʾmtʿsm, exactly as in RÉS 3962/3-5 (for this text, cf. JaMAR, III, pp. 74-75). And "NFSHYSM/...M/GLM"(l.3) is also found in a similar pericope in Ja 867/2, viz. nfshyw/klsm/gm.

For Pirenne's interpretation of bḥg/ʾnby as "avec attestation de préavis," cf. CIASA, pp. 121-123. But, the name of the god ʾAnbay is so well-known that it cannot be interpreted in any other way without an obviously irrefutable reason which Pirenne does not have; her reason is nothing but her dreamy theory. Furthermore, why and to whom should a "previous notice" be given by someone who wishes to build a house? C. Robin's main reason for rejecting Pirenne's interpretation is that "si 'nby était un substantif, on s'attendrait à le trouver avec mimation" (cf. Raydān, 4 [1981] 1982, pp. 72-73).

Robin still ignores the well-known fact that the usage of the mimation is not

symbols indicate the author of the text as a Qat ruler (cf. commentary on Ja 2976, to be published).

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1 rydᵓl/wyśfᵓl/nᶜmwd/ḏtw/ḏrṯn/śᵓmw/wbrᵓ/w

2 ṣrb/bythmy/ṣrl/wᶜḫṯbs/wṣrḫtsww/wᶜmtᶜsm

3 wnfshysm/klsm/ḳlm/bḫg/ᵓnby/wᶜl/tᶜly/bᶜṯtr

4 /wᶜm/wᶜnby/wbśmm/ytᶜn/wḳrmn/wbḏt/ṣntm/wb

5 mrᵓm/wrwᵓl/ḏyln/yhnᶜm/wᶜḫhsww/ᵓmlk/qtbn

1 Raydᵓil and Yaśafᵓil [of the family] Naᶜamwadd, the two men of [the clan] Darḥān, have bought and constructed and

2 secured title to the house of them both, Garal, and its workshops and its upper rooms and their guest rooms

3 and their roof terraces, all of them together, according to the law of ᵓAnbay and ᵓIl Taᶜalay. By ᶜAttar

4 and ᶜAmm and ᵓAnbay and Ḥaśamum Yaṯaᶜān and Ḥarmān and by Dāt-Ġantim and by

5 their lord, Warawᵓil Ḏaylān Yuhanᶜim and his brothers, the kings of Qatabān.

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L.3: wnfshysm: RyESA (IX, p. 117) translates the word as "objets mobiliers" on the basis of Ar mataᶜ (not mᾱtaᶜ) "household goods, utensils and furniture." This interpretation is difficult to accept because, here as in Groom-Beihan 19 a and RES 3962 (see below, the appendix), nfsᶜ is a part in a series of words, each of which indicates a section or building of a house, and all of them depend at least on two verbs, among which is brᵓ "to construct." It seems obvious that nobody "constructs" "household goods." I therefore suggest referring nfsᶜ to Daṯinah mataᶜ "guest" (cf. JaGD, p. 2675) and translating the word as "guest room." - klsm, cf. JaMAR, I, pp. 16-17.

L.4: bḫmm, e.g. TC 1335/6 (cf. JaNPIO, p. 54). - Yṯᶜn is here a divine epithet which most probably is the name of a temple consecrated to the god. - ḫrmn, e.g. RES 311/4.

L.5: wrwᵓl/ḏyln/yhnᶜm, e.g. Ja 122/6. - ᶜḫhsww, AM 736/9. In this text and here, ᶜḫ introduces a plural.

Pirenne's published photograph has no scientific value since she admits that "j'ai repassé les lettres visibles" (p. 230). Furthermore, how can anyone justify or even explain the metamorphosis of "YGR³L" of her transliteration of l.2 to "YGRṢM" of her translation? How can the editor of a Qat inscription be as incompetent as to release the transliteration of a pericope as full of errors as "W³H/BḌS/WSRḪŠSWW/W³MTᶜN/Dᵐ" (l.2) which undeniably is a misreading of

w³h ṯ s/wārḫtsww/w³mtᶜsm, exactly as in RES 3962/3-5 (for this text, cf. JaMAR, III, pp. 74-75). And "NFSHYSM/...M/GLMᵐ" (l.3) is also found in a similar pericope in Ja 867/2, viz. nfshyw/klsm/gm.

For Pirenne's interpretation of bḫg/³nby as "avec attestation de préavis," cf. CIASA, pp. 121-123. But, the name of the god ³Anbay is so well-known that it cannot be interpreted in any other way without an obviously irrefutable reason which Pirenne does not have; her reason is nothing but her dreamy theory. Furthermore, why and to whom should a "previous notice" be given by someone who wishes to build a house? C. Robin's main reason for rejecting Pirenne's interpretation is that "si 'nby était un substantif, on s'attendrait à le trouver avec mimation" (cf. Raydān, 4 [1981] 1982, pp. 72-73).

Robin still ignores the well-known fact that the usage of the mimation is not

that rigid, and his reasoning is valueless.

Finally, Pirenne is so blinded by her self-confidence that the letters or word restored in her transliteration of ll.1 (three letters), 4 (one letter) and 5 (one word) become absolutely certain in her translation.

In other words, the result of Pirenne's publications is negative. Her incompetence to establish her final text is unquestionable; yet, it is upon such a basis that she "a repassé les lettres visibles." Horribile dictu! Therefore, her retouching of the photograpah is wholly unreliable, and her photograph which could have been used is now completely useless. What a pity that the reader finds himself in the position of being unable to check Van Lessen's copy letter by letter and, if and when necessary, to correct his reading!

X - PiDPH 24 (cf. facsimile on p. 235) is a large panel located in "wadi ʿAbadan sous le Jabal Malaḥa" upon which four inscriptions are engraved. The coordinates of Wādī ʿAbadān are "14°29'N - 46°29'E" in GaPDRY, p. 1A. The wādī is located southwest of Niṣāb; cf. also Pirenne's sketchy map in her fig. 1. Incidentally, note that the page with the map is not, and cannot be, numbered since the first page of her written paper is on p. 205, and p. 204 belongs to the article preceding hers.

On the inscribed panel, Pirenne writes this: " la grande inscription sous le Jabal Malaḥa, que j'ai découverte le 1er mars 1976(46) et dont j'ai pu améliorer la lecture, cette année, en prenant une série de photos au lever du soleil. Je donne ici ma transcription (fig. 5) mais j'ai confié à Ḥamud Saqqaf, qui m'accompagnait, le soin d'améliorer peut-être encore la lecture, sur place, et d'en donner la traduction." (p. 236)

It is obvious that Pirenne did not decipher the inscription from the original rock, but only from two sets of photographs. It is inexplicable, in my opinion, that, having planned her return to the same region and, therefore, being in a position of making herself her own itinerary and her own timetable, she allotted to that important epigraphical panel just enough time to take "une série de photos au lever du soleil" and apparently at the tail end of her trip as if it were a mere accessory appendix, and her paper reflects the same general attitude: the mention of that inscription covers less than six lines almost at the tail end of her paper. Her apparent reason for such a general attitude was that she at all cost and above all wanted to give herself the satisfaction of "retourner sur place pour une véritable enquête" (p. 205) ... which would enable her to prove that

"Monsieur Audouin" (p. 205) was wrong in his interpretation of "Khasinet ed-Dari" and that her own was correct. Summarily, by narrowmindedly seeking her own personal satisfaction on a secondary archaeological question, she voluntarily sacrificed what would actually have been her very best contribution to SA and, ultimately, she brought shame upon herself by publishing a facsimile (p. 235, fig. 5 bis, not "5" as in her above-quoted statement) so incomplete and inaccurate that her claim as a professional epigraphist is irremediably jeopardized.

Texts A: the main text composed of 44 fragmentary lines;

B: a short three-line text below and to the left of the lower left corner of the preceding inscription; and

C and, immediately to its left, D are two short texts composed of three lines each.

1 - Text A.

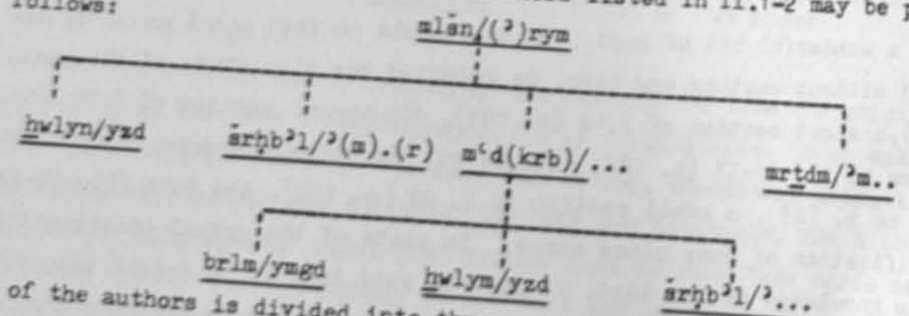
Even at first sight, the very fragmentary copy made and released by Pirenne contains many errors, a few of them had already been pointed out by M. Bafâqîh in Raydān, 4 ([1981] 1982, p. 29). Pirenne states that "j'ai confié à Ḥamud Saqqaf ... la traduction" (cf. above, p. 44). Surely, she did not really think that her copy would be improved and certainly not extensively; otherwise, why would she publish many errors, and why did she not wait for Saqqaf to finish his job? At any rate, C. Robin ... pushed both Pirenne and Saqqaf aside.

He states that " j'ai eu la chance de pouvoir examiner cette inscription pendant deux jours, les mercredi 14 et jeudi 15 novembre 1984. Grâce à la copie faite sur place et à des photographies prises avec différents éclairages, de nombreux compléments et corrections ont pu être apportés au relevé de Jacqueline Pirenne, notamment le nom des auteurs et la date." (cf. PSAS, 16 [1986], p. 182).

It was indeed a wonderful bit of good luck to stumble on that panel as if by accident ... and without wasting any time, he released the titulature of the authors (ll.1-2), a short section of l.14 (p. 183), the second section of l.29 (p. 182), a section of ll. 36-37 (p. 184, note 4; but the crossreference to this note is missing on p. 183), a small section of l. 42 (p. 182), the date (ll. 43-44) and his identification of many place names. In spite of the actual contribution brought to the knowledge of the text, it must be said that it is indeed shameful and haughtily selfish for anyone to so dissect into small pieces an important inscription for his own vain glory, instead of releasing its full text, and flinging the pieces to scholars as if scholars were mere dogs which the author intended to momentarily quieten by throwing at them a good amount of pacifiers, viz. some well-chosen morsels along with the head and the tail of the kill; in final analy-

sis, the reader is at a loss for knowing exactly what of Pirenne's copy remains accurate. Furthermore, Robin speaks of the text "encore inédit" (p. 181) But, as a consequence of his undoubtedly well-planned initiative, the reader is left in the dark as to the person who ultimately is going to publish the text. Moreover, apparently playing upon the word "inédite," Robin convinced himself that he is complying with his own verdict, viz. "une publication d'inscription sans photographie est inutile" or "seule la photographie est fidèle;" for that question, cf. JaMAR, XIV, pp. 271 and 279-280. In fact, he is giving another example that he makes his so-called verdicts, which are as extravagant as his transliteration system, for ... others. Yet, he keeps contradicting himself; e.g., he does not publish any photograph or even a copy of both RoDAA 6 and RoBaDNIR 3 (cf. Raydān, 4 [1981] 1982, pp. 51 and 84, respectively). Finally, Robin's method of handling a text is also prejudicial to research because it plagues scholars with unnecessary and unappreciated problems or question marks; e.g., (1) Robin speaks of "un member de la famille royale qualifié de 'roi'" (p. 183), (2) Robin quotes seven words of "ll. 39-40," but, at the same time, his summary of the very same place, "ll. 39-40," demands a text entirely different from that of his quotation (p. 183); (3) Robin states that "les qayls ... capturèrent ou tuèrent 50 lions" (p. 183). Furthermore, Robin introduces in his text unusual, strange translations which remain without any justification; e.g., "w-hrgw bqr^{m} w-ḥwry^{m}" is translated as "on tua des vaches et taureaux sauvages" (p. 183). For ḥwry = ḥwrw, cf. below, IV E).

The pedigree of the authors listed in ll.1-2 may be presented as follows:



The list of the authors is divided into three sections, viz.

1 - the main author mlšn/(ʾ)rym;

2 - the names of his four sons; and

3 - the names of the three sons of mlšn/(ʾ)rym's third son.

The actual value of bnyhw after mlšn/(ʾ)rym is that of a hinge to introduce the names of the author's sons and grandsons; it means his progeny, not his children.

Furthermore, the well-known habit of those Yazʾanites of having homonyms in the same family is attested here also, viz. ḫwlyn/yzd as both mlšn's son and his own nephew; šrḥbʾl/ʾ... could also be the homonym of šrḥbʾl/ʾ(m).(r). Finally, JaMAR (XIV, pp. 69-70) denounced once more Robin's fancy and arbitrary transliteration system, and especially his use of gimmicks above letters; here the question mark and the small circle, the value of which remains unknown; it is almost like the practice of "Vive la folle du logis!" Examples in the list of authors: "'rym" and "'m.r". And, as usual, the gimmicks, which have never been explained, disappear in the translation, viz. "'rym" and "'m.r."

The list of the authors of the text introduces the first pericope of the inscription, viz. sṭrw/wtryd/ḏn/m[śnd]n have written and desired this ins[cription]. The description or even the summary of the activities described in the inscription, viz. the military campaigns, the constructions and the hunting expeditions, must wait until the full text is released before they can be assessed in the whole context.

Preliminary note on texts B-D. - Robin does not mention the presence of those three inscriptions located on both sides of the lower end of the text because they are not what he is after, the "spectacular."

2 - Text B: on the lower left corner.

wʿkʾtʿbʾ/ḏn/m	1	And has written this ins-
śndn/ʾlzʾd	2	cription ʾIlzaʾad,
grʿbʾyʾn/bn/yḥḏb	3	the Garbite, son of Yaḥḏub.

3 - Text C: on the lower right corner.

wʾ(bm)/bn/ʾlšrḥ	1	Waʾ(bum), son of ʾIlšaraḥ,
qdm/(mbr)y/ḏn/mś	2	has directed the (engrav)ing(?) of this ins-
ndn	3	cription.

wʾ(bm): the third letter is traced in the facsimile as a y with its upper appendix as an upside-down isosceles triangle instead of a circle as in l.2 and in the three other texts. Furthermore, the tracing shows a vertical staff in very faint stippling perpendicular to the right edge of the above-mentioned triangle. The fourth letter is in very faint stippling; cf. wʾb in Ja 2774ci. — (mbr)y: the facsimile shows the first letter as a doubtful l: a vertical staff nicely traced with a very faint triangle appended to the upper left of the staff. This letter is followed by rby which could have been written instead of bry; cf. the well-known bry[/]šms.

4 - Text D: between texts A and C.

w^{ɔ}lm/bn/hrbḫ/w<.>	1	Wa^{ɔ}ilum, son of Harbaḫ, has
sm/^{ɔ}sṭr/ḏn/mśndn	2	traced the lines of this inscription
l^{ɔ}mr^{ɔ}hw/^{ɔ}lht/yz^{ɔ}n	3	for his masters, the people of Yaz^{ɔ}ān.

The facsimile shows after the second w of l.1 a faint ʿ as if it belongs to the text. It must represent a feature in the stone.

Texts B-D deal with the material aspect of text A. This long inscription was composed and worded (ktb) by ^{ɔ}Ilza^{ɔ}ad, the author of text B; the engraving itself (wsm/^{ɔ}sṭr) was the work of Wa^{ɔ}ilum, the author of text D; and the overall supervisor (qdm) of the engraving ((mbr)y) was Wa^{ɔ}(bum), the author of text C.

5 - W.W. Müller's paper on "Mahra" (cf. Encyclopaedia of Islam, New Edition, Leiden, VI, fasc. 99-100 [1986], pp. 80A-84A) utilizes text A. The epigraphical section of Müller's prose, the only part which is concerned here, is well in the line of other examples presented in JaMAR, XIV, pp. 132-154, viz. it contains more examples of his slanting, confusing and inaccurate way of argumenting which seems to suggest that he does not really understand the words he uses.

a - Müller's first and central argument is based on RÉS 4877, republished as Ja 954, ll.2-3: kbr/^{ɔ}mhrn. His conclusion reads as follows: "Thus, ^{ɔ}mhrn is the nisba of an unattested* mhryn 'Mahrite'" (p. 81A). It is not superfluous to point out that, contrary to Müller's statement, "mhryn" means the Mahrite, not simply "Mahrite." Furthermore, and again contrary to Müller's position, his statement is not the conclusion of his argument (cf. his "Thus"); it simply is the enounciation of his theory which remains to be proven.

b - Indeed, the s e c o n d phrase of his argumentation reads as follows:

> " in the inscriptions, *kabīr* almost always indicates the leader or the chief of a tribe, and is followed by the name of the tribe or by the *nisba* plural in the form *ʾfʿln*, *afʿūlān*, usual in ancient South Arabian "(p. 80B).

(1) This is another example of the attitude that pesters so much SA studies : the meaning of a word depends on the whim of the authors and the need of their study at any definite time. Beeston claims that kbr is connected with a place name (cf. above, pp. 8-9); now it introduces a tribe name according to Müller!

(2) Were Müller be guided by good logic, he would have seen that his adverbial expression "almost always" should have forced him to present his conclusion as a hypothesis, not as a certainty.

(3) The SA plural form ^{ɔ}qtl is very com-

mon and has nothing to do with a nisba; e.g., here are already five examples of it in the first full page of JaSIMB's glossary (p. 427), viz. ^{ɔɔ}dn, ^{ɔɔ}wd, ^{ɔɔ}lf, ^{ɔɔ}nṯ and ^{ɔɔ}rḫ. Therefore, the simple mention of such well-known fact already proves by itself Müller's intent to mislead his reader.

(4) Müller's preceding statement is immediately followed by these five examples: " , e.g. kbr/fyṯḫn (RES 3913,1) "kabīr (of the tribe) of Fayṯḫān", kbr/ṣrwḥ (RES 3951, 1 f.) "kabīr (of the city-tribe) of Ṣirwāḥ", kbr/kl/šʿbn/^{ɔ}rymn (RES 4085,1) "kabīr of the entire tribe of the Raymānites", kbr/ḥśrn (Ja 816,2) "kabīr (of the tribe) of the Ḥaśirān", etc.; the "leader of the Bedouin of the king of Saba^{ɔ}" (kbr/"rb/mlk/sb^{ɔ}: Ja 665, 1 f.)" (pp. 80B-81A).

There cannot be any doubt that Müller chose and copied his five examples with the greatest care since they are intended to prove his statement.

(a) It should be emphasized that all those examples are Sab, but that the text he is dealing with is Ḥaḍr.

(b) Most importantly, Müller does not give a single example of what he proclaims to be an evidence, viz. the expression kbr + a plural nisba. Indeed, ^{ɔ}rymn of RÉS 4085/1 (his third example) is a proper name, not a nisba, as it is introduced by šʿbn the tribe which is followed by a proper name in apposition. The proper name ^{ɔ}rymn is also a part of the compound clan/tribe name mḥfdn/^{ɔ}rymn in RÉS 4636/4. Finally, Müller's translation of kl/šʿbn/^{ɔ}rymn is voluntarily misleading because his "the entire tribe of the Raymānites" should normally represent an original as kl/šʿb/^{ɔ}rymn, viz. šʿb instead of the epigraphically attested šʿbn.

(c) Müller distorts and misunderstands his fourth example: his "kbr/ḥśrn" of Ja 816/2 is an error instead kbr/^{ɔ}ḥśrn, as clearly transliterated in JaSIMB, p. 241B; cf. also the photograph on pl. 48, upper right corner. Therefore, the "Ḥaśirān" of his translation also is erroneous. The interpretation of ^{ɔ}ḥśr of Ja 816/2 as a plural next to ḥśr (e.g., RÉS 4798) designating a class of important persons is all the more appropriate in that it is followed by the well-known ^{ɔ}qyn administrators, and that the same word precedes mśndn/bklythmw the council in their full strength in CIH 601/4. In all those instances, there is no question of a clan or a tribe at all.

(d) In his fifth and last example, Müller introduces immediately after kbr a word divider which does not exist. Furthermore, the personal substantive ^{ɔ}ʿrb is the plural of ʿrb and is a generic

noun having no relation with a clan or a tribe.

(e) Since Müller does not care to give even a single example justifying his "almost always," here follow a few examples for the sake of the reader:

Sab: kbr/ʾrš/mtbnṭyn (RÉS 3303/2-3) leader of the priests of Mutībnaṭyân;
Ḥaḍr: kbr/ḥḍrmt (Hamilton 2/2) leader of Ḥaḍramawt, viz., the country, not a tribe;
Min: kbr/ytl (e.g., RÉS 3403 A/4) leader of Yatil, viz. a city, not a tribe;
Min: kbr/ʾdm/mlk/mʿn (RÉS 2976/2-3) leader of the servants of the king of Maʿīn;
Qat: kbr/tmnʿ (RÉS 3854/6 and 8) leader of Timnaʿ, viz. a city, note a tribe,
Qat: kbr/mʿnm/btmnʿ (VL 9/1) leader of Maʿīnum in Timnaʿ. mʿnm refers to the Min residing in the Qat capital, not to the tribe itself. Those Min belonged to many different clans and tribes; the author of the text, himself, belonged to the clan rwyn Rawyân (cf. above, pp. 4-5).

The conclusion to be drawn from the preceding presentation is precisely the opposite of Müller's theory, viz. kbr followed by a plural form, such as ʾqyn et ʾḥśr, governs a generic substantive which is the appellation of a class of persons, e.g., ʾdm, ʾʿrb, ʾrš or ʾmhr.

(f) It may well be that it was almost at the same period Müller composed his paper on "Mahra" that he wrote another paper published in 1985 (cf. "Altsüdararabische und frühnordarabische Inschriften," in O. Kaiser, Texte aus der Umwelt des Alten Testaments, Bd. 1: Rechts- und Wirtschaftsurkunden, historisch-chronologishe Texte, Güterloch, pp. 651-668).

This paper shows Müller's opportunism by which he adapts his theories to the circumstances, his lack of good common sense by contradicting himself, and also his gullible attitude toward the publication he likes. His new definition of kbr is as follows: "Vorsteher, Führer eines Stammes bzw. einer Stadtgemeinde" (p. 663, note 1a). Where is the restrictive expression "almost always" of the first definition? For sure, his erroneous concept of the tribe chieftain is maintained, but that concept is further specified by "bzw. einer Stadtgemeinde" = "or rather of a municipality." He would obviously enough never have given such a definition when dealing with the tribe of Mahrat, and that definition was already contradicted by the last example of his own choice, viz. kbr ʾʿrb/mlk/sbʾ. However, who knows, all those bedouins of the king of Sabaʾ might have been living in a "Stadtgemeine" called "Müllerstadt" which is still unknown in SA epigraphy and archaeology! Müller mentions his new theory because he is commenting on the Min expression kbry/mqrn/wmʿn/mqrn of RÉS 3022/1. He does not even see that he contradicts

himself by implying that mʿn was a "Stadtgemeinde." He translates the word mqrn as "Masran" [sic!] in both places but his note 1b reads as follows: "Oder: der Grenzmark; ein Gebiet in Nordwestarabien, wahrscheinlich die minäische Kolonie in der Oase von Dedan and der Weihrachstrasse."

The idea of "Grenzmark" comes from Ar miqr "a partition, barrier, or thing intervening, between two things ... as also miqâr or limit, or boundary between two lands" (cf. LaAEL, p. 2719B). Of course, Müller ignores that the word mqr in well-known in SA epigraphy; it means "expeditionary force, expedition abroad;" cf. JaSIMB, p. 440B and pp. 72B-73A, where it is stated that in "mʿn/mqrn (e.g., RÉS 3022/1, Min) ... mqrn is a proper name, but could have originally meant 'the expeditionary corps'." The word mqrn designates the territory where the Min expeditionary corps settled down, in and around Dedân. The commanding officer could be called kbr because the occupied territory was considered as belonging to metropolitan Maʿīn, and the second kbr of the text was the leader of the Min population residing in that territory, viz. inside and outside Dedân (cf. also above, p. 8). Incidentally, the name of the second kbr's father is given by Müller as "ʿAllan," thus, ʿln, a name which he copied from a book that contains what he calls the "jüngste Publikation des Textes in Umschrift." This statement calls for the following remarks. (a) The book in question is A. Capuzzi, Iscrizioni minee, Napoli, 1975. Müller fails to give the name of the author although p. X of the book clearly states that "questo volume, contenente le iscrizioni minee, è stato curato della dott.ssa A. Capuzzi."

(b) The book in question is not a publication of texts; it is a simple compilation of several books and papers without any research to establish the accurate decipherment of the texts; it contains an amazingly low number of pages devoid of any mistake. (c) RÉS's transliteration has "wlg" (V, p. 304) on the basis of E. Glaser's copy, while Capuzzi has "ʿln" (p. 71), which is K.Y. Nâmî's reading, but Capuzzi does not give any reason to justify her choice of Nâmî's reading against Glaser's copy. The two Glaser squeezes, A 195b (upper right corner) and 195c (upper left corner) show that the name is wlg.

c - Müller's third statement is that he would vocalize ʾmhrn as "Amhūrān, or perhaps Amharān, since the place-name Burkat al-Amhār, mentioned as lying next to al-Ghayda (Mahrī: Ghaydat) (Hamdānī, *Sifa*, 147, l. 17), certainly does not mean anything else but "Burka of the Mahrites". " (p. 81A).

As it stands, the Ar expression burqat al-ʾamhâr means the rugged terrain of the colts. ʾamhâr indeed is the plural of muhr colt; Ar mahârîy, plural of mahrî

Mahrite and even the two Mehri plurals, mahrê and mehrê, which he himself acknowledges (p. 80A), contradict Müller's interpretation. It seems certain that Müller, fascinated by the similarity of the three consonants mhr, never asked himself how a SA form would be found on a non-SA soil.

(d) Müller concludes his argumentation on Ja 954 by invoking W. Dostal's testimony. For Dostal, it is "zweifellos" that many Mahrites served as "Söldner im Hochkulturbereich" because of this simplistic reason: "Eine Erscheinung, die wir bei vielen Randvölkern beobachten können" (cf. Die Beduinen in Südarabien, Vienna, 1967, p. 134). Surprisingly enough, Dostal's "zweifellos" becomes in Müller's prose "Dostal's conjecture," and Müller states that Dostal's theory "is confirmed by inscription RES 4877" (p. 81A). If it is understandable and, to a great extent, expected, that an ethnologist would let himself be guided by general considerations without inquiring whether or not they actually are applicable in his particular case, Müller's assent seals off the case of his incompetence on the kbr question. All the contexts mentioning kbr indicate that the kbr function was always exercized upon persons belonging to the same tribal or political entity as that of the author of the text; e.g., the Min ʾAwsʾil, son of Laḥayʿaṯat, is kabir of his own fellow countrymen residing in the foreign capital of Qatabān (cf. VL 9 mentioned above). Furthermore, there is no epigraphical evidence suggesting that the Mahrat country ever became a part of the Ḥaḍr kingdom. The foundation of "Sumhurâm" on the Dhofar coastline was aimed at the incense wealth in the northern mountains, and does not suggest the annexation of the hinterland east of Mukallah. That Ḥaḍr foundation was parallel to the Min colony at al-ʿUlâ, which did not imply the Min conquest of the whole country between Maʿîn and its northern outpost. It was even much easier for the Ḥaḍr to reach Sumhurâm than for the Min to reach al-ʿUlâ, as they had free access to the sea to go back and forth to their foundation.

e - Müller's fifth statement is to the effect that

> "A further evidence for the country and the tribe of Mahra has been found in the Sabaean rock-inscription from Wâdî ʿAbadân from the middle of the fourth century A.D., in which military campaigns "towards the country of Mahra" (line 7: *qbl/ʾrḍ/mhrt*) and "against the Mahra" (line 21: *ʿly/mhrt*) are mentioned" (p. 81A).

But the existence of mhrt does not need any proof, and its mention in a SA inscription does not prove either the annexation of the territory of mhrt nor Müller's interpretation of Ḥaḍr ʾmhrn. Pirenne's facsimile, indeed, has qbl/ʾrḍ/mhrt(1.7)

and ʿly/mhrt (1.21). Robin does not allude to the first reference (p. 182), but lists the second as "Le Mahra (Mhrt, l. 21-24)" (p. 183). L.7, however, puts ʾrḍ/mhrt rather close to ybrn, which Robin identifies with "Yabrîn ... en Arabie orientale" (p. 182). As everybody knows, homonym place names do exist, as exemplified by mrb, which is Mârib of Sabaʾ, and Marab of southern Kindat (Ja 576/2), and only a close study of PiDPH 24A may be able to show whether mrb of ʾbʿl/mrb (l. 18; not mentioned by Robin) is yet another place. This is the first example illustrating how shameful it was to release scraps of that long text. The preceding examples prove again that material consonantic identity of two names, albeit very tempting, does not necessarily imply formal identity.

(f) Müller sixth and final statement is to the effect that

> "An alleged form *mhrt*, found in the late Sabaean inscription RES 4069,5 from Niṣâb, has nothing to do with the Mahra; the passage should rather be read as *wmḥrg/wkbwr/šʿbn/sybn*, and translated as "and the administrators and the leaders of the Saybân tribe"."(p. 81A).

First of all, RÉS 4069, the copy of which was made by M.J. Boscawen, is engraved "sur le rocher qui longe le Wâdî," viz. W.Ḍuraʾ, and "la localité de Niṣâb se trouve au confluent du Wâdî Ḍurrâ et du Wâdî ʿAbadân" (cf. Le Muséon, 45 [1932], pp. 289-290, and the map in fig. 1 of PiDPH). The text, thus, does not come from "Niṣāb," as repeated by Müller after RÉS, VII (1950), p. 66. Furthermore, Müller should have been honest enough to inform his reader than he is not the first to suggest the correction he mentions; RÉS (l.c., p. 67) had already made it, viz. " L. 5. מחרת, lire מחרג «suzerains» ? Cf. *R.É.S.*, 2633, 6.", viz. wkbwr/wmḥrg/sybn, the pericope of RÉS 2633/6 is also found in RÉS 4069/5 as wmḥrg/wkbwr/šʿbn/sybn. Thus, the only discovery made by Müller is to read it well explained in RÉS. Finally, although admitting that which is obvious, viz. that the text of RÉS 4069 is known only "par une médiocre copie de Boscawen" (cf. Raydân, 1 [1979], p. 81), Robin refuses to acknowledge the parallelism between RÉS 2633/6 and 4069/5, and still maintains that "Mhrt, le Mahra, ... est mentionné dans RES" 4069/5 (cf. PSAS, 16 [1986], p. 185).

IV - Notes on J. Pirenne's 1977 book.

If the reader takes into consideration Pirenne's incompetence and blindness such as exemplified in the preceding pages and especially by her facsimile of PiDPH 24A, he is well prepared to adequately evaluate her following statements which are well worthy of reproducing at length. They are part of her study of Ja 2353 (1) and read as follows:

> "Quant au R. P. Jamme, il prétend tout lire, et sans laisser de doute, comme le montre son dessin ... Mais la traduction qu'il avance suffit à juger et à condamner cette méthode qui prétend lire tout, même l'illisible, et traduire tout, même si le résultat obtenu ne présente aucun sens, mais oscille entre la bizarrerie et le romantisme, et où les mots difficiles sont trop facilement transformés en noms propres" (2).

What else could be expected from Pirenne who still resents that her basic 1956 work on SA palaeography not only was rebutted in a 198-page review (3) but also was steadfastly rejected all through the subsequent years while she was flattered, her work praised and her working method extolled by both G. and (naturally) J. Ryckmans (until he turned against her after her star had fallen into disfavor) as well as by the Paris-Brussels axis. Blinded by an imagination which she apparently is unable to master or dominate, Pirenne sees what she wishes to see; invents what she wishes to find; and finally closes her eyes to what she does not wish to see (4).

A - Here follow four typical examples of Pirenne's working method taken at random from the same book among those that do not demand a long commentary.

1 - On p. 199, she reproduces F.P. Albright's plan of the oval enclosure of the Maḥram Bilqîs and claims that "la situation des inscriptions sur le mur ... a été ajoutée par nous."
Had she checked with JaSIMB's plate C, upper left corner, she would have noticed - it is to be hoped - that in her sketch she omits two texts, Ja 552 and 553.

2 - On the same p. 199, Pirenne characterizes "tous les textes du côté ouest" as "situés sur une même assise de pierre."
Such a statement is so obviously wrong that it can only come from a disturbed

mind. Not only all information is clearly given in the introduction to each text, but the schema of the location of the inscriptions is plainly printed on JaSIMB's p. 260.

3 - On pp. 199-200, Pirenne deals with a pericope of Ja 551, viz. kl/mǧbb/wmḫfdt/bᶜly/mhyᶜn, which is translated by JaSIMB as follows: all the recesses and towers on this wide wall" (p. 15B). Pirenne claims that my translation reads as follows: "all the recesses and towers of this wide wall" (p. 200), and she immediately reproaches JaSIMB with two errors, viz. "la traduction 'wide wall' pour MHY [sic!] omet la préposition BᶜLY, et élimine le problème sous une expression banale."

Unfortunately, she copied and understood "of" instead of the original "on;" and her two critics are based only on her erroneous transcription of the original text. Then, she adds: "quant aux 'tours et rentrants', il est clair que le mur n'en a pas."

What is clear is that the upper part of the wall is missing and, unless she received a revelation on the question - if she did, the reader would like to know where it came from -, Pirenne may imagine, if she wishes, whatever she wants, viz. that there was no tower at all, but nobody is forced to believe in her dreams. The commentary of JaSIMB states that mǧbb "refers to the parts separated from each other by towers," and mhᶜy "refers to the wall and emphasizes the thickness of the masonry" (p. 18). Where is the difficulty? Incidentally, note that the photograph on pl. 27 is said to have been taken "auprès du temple ovale de Ṣirwāḥ" instead of "à Mārib," as it is stated on p. 214.

4 - On p. 217, where she refers to the word nkl of Ja 671/17, she notes this: "A. Jamme a traduit par 'artistic work' sans justification." JaSIMB's introduction states in clear terms that "in the commentary which follows the translation, the study of a term is normally given at its first occurrence in the collection" (p.4).

Of course, Pirenne could not care less about what an author may write unless she wishes to please him/her or she is seeking either to conquer or to preserve his/her friendship. For Pirenne as well as for the laudemus invicem club members, scientific research has long become the maidservant of personal politics. Pirenne did look at JaSIMB's glossary since she acknowledges that nkl is found in "Ja 560 et 671" (seven lines above her quoted statement). Therefore, why did she not look in the commentary of "the first occurrence in the collection"? She would have found the ten lines devoted to the etymology of nkl on p. 33B. Summarily, when reading a text, she gives the best of her attention to her own theories and the

ways to promote them instead of to the text itself so that she may understand it properly.

B - Here follows the photostatic copy of my review of her book in which Pirenne published the above-reproduced quotation:

J. Pirenne's latest work, La maîtrise de l'eau en Arabie du Sud antique. Six types de monuments techniques (Mémoires de l'Académie des Inscriptions et Belles-Lettres, Nouvelle série, tome II, Paris, 1977; cf. a short summary in Raydan, 1 [1978], pp. 71-73) is the most fantastical study ever produced by the author and may be considered as the embodiment of her method. J. Pirenne's latest work is absolutely in the line of all her preceding publications, showing the very same style, reasoning and argumenting. Then, J. Ryckmans' initiative of denouncing and deriding without any delay her etymological "jongleries" as well as "toute la méthode de l'ouvrage" (cf. Le Muséon, 91 [1978], pp. 252-256) truly is, at least to the ordinary reader, a puzzling enigma shrouded in gloom because, in the past, he practically endorsed faithfully at one time or another all J. Pirenne's theories, e.g., in art, history, palaeography and religion. For instance, in his review of J. Pirenne's book Le royaume sud-arabe de Qataban et sa datation... (Louvain, 1961), J. Ryckmans was full of praise (cf. BiOr, 21 [1964], pp. 376-378): "Voici un livre qui ne passera pas inaperçu" (p. 376A), "la démonstration nous paraît entièrement concluante en faveur de la chronologie courte" (p. 376B), "l'auteur voit sa clairvoyance récompensée" (p. 378A), and "On ne peut ... que souhaiter que Mlle Pirenne puisse rapidement mener à bien la seconde partie de sa Paléographie"(p.378B). Since he was such an admirer of J. Pirenne, the least J. Ryckmans owed to his reader was to explain how and why, in his opinion, her entire method has so suddenly deteriorated so drastically in her very last work; in other words, how and why, all through a volume of 237 pages, her celebrated "clairvoyance" has led her so completely astray, and how and why her admired "démonstration ... entièrement concluante" has so lamentably failed. Whether or not his past endorsements were meant as they read - it is up to each individual reader to answer the question -, J. Ryckmans' recent 180° turnabout raises the same questions which I asked in 1955 in De la méthode en épigraphie (Washington, p. 22), viz. "quelles susceptibilités [has J. Pirenne] ainsi réveillées? Quels intérêts, quels projets, peut-être [has she] contrariés?" The ordinary reader will never know. Furthermore, it will be most interesting to see how J. Pirenne's past admirers are going to extricate themselves from their former endorsements of her theories and opinions. Finally, G. Garbini gave at the occasion of his review of J. Pirenne's latest book mentioned above a good example of his knowledge in SA matters by opening his comment by this phrase: "Con 'La maîtrise...' Jacqueline Pirenne ci ha dato il più straordinario dei suoi libri e forse la più acuta indagine che sia stata compiuta finora sull'antica civiltà sudarabica" (cf. AION, 38 [1978], p. 107). (5).

C - A.F.L. Beeston's review of Pirenne's book (6) is mainly devoted to philology, and his judgment is that "in dealing with the linguistic part of her work..., she displays a lack of sound philological method which is little short of disastrous" (p.101).

1 - To understand Beeston's attitude, the reader must keep in mind that J. Ryckmans' 1978 review had clearly manifested that Pirenne's idyllic overall status or honeymoon had suffered a serious setback, which justifies and

explains his own about-face, and then, therefore, it would no longer be dangerous or even unadvisable to attack her violently, but even that such a move would somehow be pleasing to those who had in any way been a part of that sour turn of events. And here, as in the case of his own about-face on the so-called "ritual hunt," Beeston waited three years before writing his review, as he had done in his handling of that question in connection with my latest rebuttal of that hunt, "hoping that" Ryckmans' review "would in the meantime have somehow been forgotten by his reader and also that he did not wish at all to refer to it" (7). Furthermore, it should be pointed out that Pirenne's philological blunders are of the same nature as those of Beeston, e.g., his phantastic interpretation of the words byt and lf[3], but that none of Pirenne's erroneous examples equals Beeston's preposterous etymology of hmtn of Ir 37/14 and 24 (8). Finally, the reader knows that neither Ryckmans nor Beeston were the first to denounce and stigmatize Pirenne's argumenting method; they simply are in the line of my thirty-year old position on the subject (cf. above, p. 54).

2 - Beeston, however, sometimes deals with archaeological matters; here follow two examples.

a -

[A] P. says "il reste encore les temples [*sic*] RŞF*m* de Ma'in, qui sont encore à découvrer [*sic*]". Nothing in the texts suggests that there was more than one sanctuary at Ma'in so named, and archaeologically there is one sanctuary which has been fully described, but shows no trace of any disposition such as she describes the *rsf* as being; her other temples "encore à découvrer" are imaginary." (p. 103). Later (p. 85),

Ad [A] - Beeston makes two errors in transcribing Pirenne's text. 1. Pirenne's "RŞFM" becomes "RŞFm." Beeston is so blindly convinced that his transliteration system is so obviously right that he carries it on even in his quotation of another writer. 2. Beeston copies twice the verb "découvrir" as "découvrer," which does not exist; and he underlines his error with a "[sic]."

Ad [B] - Such a statement is irrelevant because all inscriptions from the city of Ma'în are not yet known. However, Pirenne's statement is unacceptable because the whole epigraphical material has not yet given evidence of two temples bearing the same appellation in the same city.

Ad [C] - Beeston's statement is inaccurate: no "sanctuary [] has been fully described" (the underlining is mine) because none has ever been excavated.

Ad [D] - Beeston's affirmation is accurate because Pirenne's theory on rşfm is wrong.

b - On the question of the standing stones of Dât-

al-Ḥağr, Beeston remarks on Pirenne's imaginative concoction that "this is all very well - except that there is no 'canal recépteur' [sic]; it is as imaginary as her temples still to be discovered" at Maᶜin" (p. 104).

The reader can easily turn Beeston's verdict all the way around against him and say "'that is all very well - except that'" you do not prove anything; you simply repeat what Pirenne said to the effect that she did not find any part of that 'canal récepteur', but you fail to express your own opinion on what those 'standing stones' actually mean." Such an attitude, however, is not enough. Indeed, Pirenne starts from a wrong premise when she writes that "puisque l'on peut conclure que la rosée avait un rôle considérable dans l'Antiquité, bien qu'il soit très amoindri maintenant" (p. 175), and this statement is preceded by the following: "Cela nous rend pour l'Arabie du Sud une image conforme à celle que le Bible nous fournissait pour la Palestine; pluie et rosée inséparables" (p. 189).

Whatever the actual meaning of the second affirmation may be, Pirenne's position is unacceptable because, e.g., Western Arabia and the Red Sea (9) states that

> "Dew in Arabia is often copious enough to take the place of rain in the economy of vegetation" (p. 182) and that "the dew (tall, tofal, nida), plentiful throughout the year, [] on which vegetation in many districts depends for its existence" (p. 164).

Yet, to my knowledge, no author has ever in the past made the slightest allusion to the kind of system Pirenne dreams about in SA, and all the examples she may present from other parts of the world, even were they absolutely convincing, would never convince anyone that her brutal application to a situation having an entirely different climate is justified and would never explain the total lack of evidence in the literature of the past centuries.

3 - The last paragraph of Beeston's review is devoted to what *looks to him as a fortunate exception to his condemnation of Pirenne's work. He states this:*

> "After all this, it is a pleasure to acknowledge that she has produced one proposal which deserves to be taken seriously: that *mtll/wṣll* in the 'Uqlah formula refers to rituals designed to produce rain; her rendering is "accomplir la marche rituelle et passer la journée". It is, however, unfortunate that she has supported this with an unacceptable argument, saying "*imtalla* n'a pas d'autre sens arabe que 'he followed his way of religion'". This, however, is Lane's gloss for *imtalla bihi*, and what Lane meant was "secutus est viam ejus in religione", not "viam suam" or simply "viam", so that the phrase is meaningless without a complement referring to someone else. On the other hand, the *Lisān* does indeed record another sense of the verb, used intransitively without complement, namely *'asra'a*,[7] which would be a far better support for P.'s idea, since in the Near East a "marche rituelle" is always performed at a rapid pace." (p. 108).

Note 7 reads as follows: " 7 Cf. Arabic *sa'ā* "run" used in the specialised sense of a ritual act. "

What makes Pirenne's hypothesis so irresistably attractive to Beeston is that it mentions a "marche rituelle" and "une incantation de noms." The idea of anything ritual always has so blindly facsinated Beeston that, had she suggested also ritual activities in connection with mḥfd, rqf, etc., Beeston would without the slightest hesitation or doubt have sided with her. And in order to make their preconcieved idea more appealing, nothing comes amiss to Beeston and Pirenne.

a - For Beeston's earlier paper on the al-ʾUqlah texts (10), cf. JaMAR, XIV, pp. 84-90, according to which it "was characterized as having fancy and inaccurate suggestions and statements" (p. 80).

b - Beeston agrees with Pirenne by failing to point out that her summary of the situation is erroneous. She writes this:

> "la formule stéréotypée [is] 'X roi de Ḥaḍramout ... MTLL/WṢLL/BMḤFDHN/ ʾNWDM/HSLQB.'" (p. 172).

How dares anyone speak of "la formule stéréotypée" when the pericope is attested only once, viz. in Ja 988/2-4? An abbreviated form is mtll/wṣll/bmḥfdhn/ʾnwdm in Ja 996/3-4.

c - The following expression, which is not even mentioned by either Pirenne or Beeston, mtll/ᶜm, is used by the king's father-in-law (Ja 957/2) and brother (Ja 986/2) and son (Ja 997/3).

The first text stipulates the act of mtll/ᶜm which is followed by this pericope:

> "(and) he [= the author, who is not the king] (fur)nished for him [= the king] the fortress ʾAnwadum with fresh provisions" (1.4).

How can anyone find any trace of a "marche rituelle" in such a context? The author, the king's father-in-law, apparently was the chief purveyor of the royal household or, at the very least, of the king's travels.

The second text has as author the king's brother, who most probably was the heir apparent of the crown, because he did apparently not have any other function.

The third text is written by the king's son in his capacity of "superintendent of the soldiers," viz. the military commanding officer of the royal convoy, and he "sojourned with the king" (1.3) to assure the safety of his father so that the latter might safely slqb give titles.

How can anyone suggest that each of those persons, the king's father-in-law, his brother or his son, "a accompli la marche rituelle" so that the king might "faire

une incantation des noms"? Furthermore, the testimony of these three texts flatly contradicts Pirenne's summary which Beeston fails to denounce, according to which the subject of mtll a l w a y s is the king.

d - Pirenne also publishes the following summary:

> "Les textes qui n'émanent pas des rois sont ceux de grands personnages qui ont suivi chacun des rois dans cette affaire. Ils disent s'être conformés à son action décrite par les deux verbes MTLL et LQB (ou SLQB). (Le verbe ŞLL disparaît.)" (p. 172).

Beeston agrees with such a presentation of the facts since he does not denounce it. Yet, such a presentation is another gross distortion of the epigraphical evidence on two questions of the greatest importance.

(1) Out of the 20 inscriptions written by persons who say to šw/yᶜ accompany the Ḥaḍr king, only three of them have the pericope hmtll/whlqb, viz. Ja 923/4, 926/5-6 and 928/6-7.

(2) To say that those personnes "se disent s'être conformés à son [= the king] action décrite par les deux verbes MTLL et LQB" is a pure invention originating from a mind that injects into the texts what it wants to find.

The bridge between the contents of the inscriptions and the second above-mentioned quotation is found in the ambiguous word "participants" of the following affirmation: "On aurait donc, selon les participants qui l' [= the king] ont suivi: 'le roi ... s'est rendu au maḥfid ʾAnwadum pour accomplir sa marche rituelle et faire une incantation de noms'."

Indeed, the authors of those texts were "participants" of the royal trip to al-ᶜUqlah because they say to have šw/yᶜ accompany the king; but Pirenne instills her own idea that they were also "participants" of the king's activities. It is hardly conceivable that a normal human being could make such self-evident an error. It is so elementary that helpers, assistants, witnesses and onlookers of personal activities of other human beings are not necessarily "participants" of those deeds? Who in his right mind would state that all persons involved in both the preparation and the ceremony, e.g., of a wedding, would be said, as Pirenne claims, "s'être conformés à [the] action décrite par" the exchange of matrimonial vows by the bride and bridegroom?

e - Here follows another example of Beeston's devious argumenting method. The Lisân (11, p. 632B) indicates that malla and some of its derived forms have the meaning of ʾasraᶜa to be quick, speedy when, as pointed out by al-Qâmûs (4, p. 683B), there is question of walking: fî l-mašyi: ʾasraᶜa. Cf. also LaAEL (p. 1349B): "ʾasraᶜa fî l-masyihi he quickened, or hastened, his

pace, or going." But, "to quicken, hasten one's pace" does not at all mean "to run." Furthermore, Beeston's statement "in the Near East a 'leisurely stroll' is always performed at a rapid pace" is so general that it must be rejected as erroneous. One example suffices here:

> " the pilgrim performs the Ṭawaf, circling the Kaʿbah seven times. The pilgrim makes the first three rounds of the Kaʿbah at a fast pace, preferably with short steps, the remainder at walking pace. " (11).

Before selecting that particular meanning of malla, Beeston should have demonstrated that the text deals with walking, and that is quite impossible. Finally, the example of his "saᶜā" mentioned in his note 7 is irrelevant to the matter at hand since the word originates from the root sᶜy which is entirely different from srᶜ. Besides, the reader did not have to be reminded, as if he did not know, the rite at the Kaᶜabah and especially when the rite involved four (out of seven) rounds "at a waking pace," an information which contradicts his own thesis.

f - Beeston's strong vindictive partisanship, which is embodied in Sabaic Dictionary (12) thanks to the slavish cooperation of some of the laudemus invicem club members, forced him, as if it were a part of what he calls "My instinct" (13), to adopt once more an illogical attitude. All the examples given by him in his review are aimed at proving that no matter what kind of text Pirenne utilizes, SA, Ar or Hebrew, she distorts and misrepresents it systematically "in order to support her linguistic theories," and that her method of argumenting always appears imaginative and irrealistic. The only logical attitude to be taken by any normal person is to adopt a systematic caution approach toward every single statement of Pirenne and, therefore, to carefully check all her affirmations against the epigraphical and/or archaeological facts. It is also a matter of record that Beeston loves juggling with theories, hypotheses, assumptions, conjectures and speculations rather than with the fundamental work of deciphering the texts, and when he indulges in this basic activity, the result is nothing but blunder (14). Beeston's ugly partisanship and illogical attitude are expressed in these statements:

> " P. has carefully studied an enlarged colour slide, and gives an extremely useful transcription based on this, which shows large parts of the text to be simply unreadable; Jamme's claim to be able to read the whole with confidence is dismissed on the grounds that it "oscille entre la bizarrerie et le romantisme". (p. 105)

The whole text of Pirenne's remark is given above (p. 54). The preceding pages

indeed illustrate Pirenne's characteristic myopia in her reading of the inscriptions as well as the aerial juggling in which she indulges to distort every datum in order to always have her own way in both archaeological matters and translations of texts as Beeston himself is fully aware; e.g., on a translation suggested by Pirenne he declares it to be "too bizarre to be acceptable to anyone" (p. 104). But, in the present case, Pirenne achieves the unachievable: her attitude is a perfect example of perfection ... such is Beeston's logic. It is rather easy to understand that a person who is thoroughly under the spell of "la folle du logis" or constantly riveted to dreamy hypotheses and preconcieved ideas, as abundantly illustrated by so many pages of JaMAR, XIV, would qualify the standards, opposite to those described above, as "bizarrerie" and "romanti^sme." It may safely be said that the disapproval of any such person in fact is a compliment of exceptional value to anyone who does not live by those aberrant standards; at least, judging from the source it came from, the reader can appreciate the standards of the person to whom it was addressed.

D - Pirenne and Ja 949.

Pirenne gives yet another detailed display of her fanciful, distorting argumenting skill in her study of Ja 949. She writes this:

> " Mais le texte le plus récent dit, selon mon interprétation [24] que le roi qui a restauré Šabwa, a aussi établi le bétyle ŠQR ṢLL au rocher de 'Anwadum, après que tout eut été endommagé par un séisme. L'installation de cette idole s'est accompagnée d'immolation d'un grand nombre d'animaux sacrifiés. "(p. 185)

Ad [A] - As shown in JaUT (pp. 10-14), Ja 949 is the earliest of the texts mentioning mḫfd since yd^{c}^{ɔ}l/byn was the founder of a dynasty attested at al-^{c}Uqlah. Cf. also below, G.

Ad [B] - Having set her mind that mtll had a ritual meaning and that each royal text reflected such a religious connotation, she distorts the meaning of three words of l.2. She erroneously translates bythn as "le bétyle." This interpretation, however, is not worse than Beeston's translation of byt as "a village with its surrounding land in the Palestinian area" (15). Then, she refuses to acknowledge two other epigraphical facts, viz. the name of the royal palace in Šabwat is šqr of bytn/šqr in ^{c}Inān 10/53 and 56 = Iryani 13/23 and 25, respectively; and in Ja 988/3 and 996/3, the two other references of ṣll prove that the word is a verb. Yet, in spite of that epigraphic material, Pirenne does not indeed shrink from stating without the slightest justification that ṣll of Ja 949/2

is the second name of her "bétyle," viz. "ŠQR ṢLL."

For the verb ṣll, I naturally refer to "paḍramoutique moderne ṣll 'séjourner' rattaché à l'assyrien ṣalālu (a) 'sich lagern, ruhen, beschlafen'" (16). Pirenne on the contrary writes: "Peut-être pourrait-on se fonder sur l'hébreu" (p. 173). Why does Pirenne "chercher midi à quatorze heures" and reject the obvious? Instead, she refers to Ar ẓll which she claims has the meaning of "'passer la journée'" (p. 173). This opinion is erroneous: cf. "ẓalla nahārahu yaf^{c}alkaḏā he passed his day doing such a thing" and "ẓalla laylahu yaf^{c}alu kaḏā he passed his night doing such a thing" (17) Thus, the words "la journée" do not belong to the translation of ẓalla.

The conclusion to be drawn from the two preceding divisions of Pirenne's quotation is that there is not much to be said when, confronted with the epigraphical evidence, an author willingly distorts the meaning of a word as well as of byt, of the word ṣll so well illustrated by the other texts, and of the name šqr. At least, such an experience removes any doubt that might still exist that Pirenne will at all cost impose her preconcieved ideas to any text...regardless of anything.

Ad [C] - The statement is an obvious exaggeration. "Tout:" it is quite certain that all the buildings of Šabwat could not have been destroyed; and the "séisme" in fact is a "collapse," whatever the cause, which the text does not specify, might have been.

Ad [D] - The statement is still another consequence of her preconcieved idea that the text must have a ritual meaning. The text, however, has hrgw they killed, which cannot be synonymous of dbḥ to sacrifice, immolate.

E - J. Ryckmans and R.B. Serjeant and Ja 949.

The full text of Ja 949 is given here so that the reader may easily follow the reasonings and argumentations of the following pages.

1 yd^{c}^{ɔ}l/byn/mlk/ḥḍrmt/bn/rbšms/bn/^{ɔ}ḥrr/yhb^{ɔ}r/dsqlb/whrr/hgrhn/šbwt
2 wbr^{ɔ}/bytn/šqr/ṣll/bgndln/mt/tbrw/bn/ṣydmn/whrgw/hms/wšlty/bqrm
3 wtty/wtmnhy/ḥwrw/whmst/w^{c}šry/ṣbym/wtmnwt/^{ɔ}fhd/bgndln/^{ɔ}nwdm

1 Yada^{c}^{ɔ}il Bayyin, king of Ḥaḍramawt, son of Rabbšams of the freemen of Yuhabi^{ɔ}r, him who had transformed and altered the city Šabwat
2 and had rebuilt the palace Šaqar, has sojourned in the fortress when they [the damaged buildings involved] had broken down because of the collapse, and they killed thirty-five bovines
3 and eighty-two young camels and twenty-five gazelles and eight cheetahs

indeed illustrate Pirenne's characteristic myopia in her reading of the inscriptions as well as the aerial juggling in which she indulges to distort every datum in order to always have her own way in both archaeological matters and translations of texts as Beeston himself is fully aware; e.g., on a translation suggested by Pirenne he declares it to be "too bizarre to be acceptable to anyone" (p. 104). But, in the present case, Pirenne achieves the unachievable: her attitude is a perfect example of perfection ... such is Beeston's logic. It is rather easy to understand that a person who is thoroughly under the spell of "la folle du logis" or constantly riveted to dreamy hypotheses and preconcieved ideas, as abundantly illustrated by so many pages of JaMAR, XIV, would qualify the standards, opposite to those described above, as "bizarrerie" and "romanti^sme." It may safely be said that the disapproval of any such person in fact is a compliment of exceptional value to anyone who does not live by those aberrant standards; at least, judging from the source it came from, the reader can appreciate the standards of the person to whom it was addressed.

D - Pirenne and Ja 949.

Pirenne gives yet another detailed display of her fanciful, distorting argumenting skill in her study of Ja 949. She writes this:

"*Mais le texte le plus récent dit, selon mon interprétation* [24] *que le roi qui a restauré Šabwa, a aussi établi le bétyle ŠQR ṢLL au rocher de 'Anwadum, après que tout eut été endommagé par un séisme. L'installation de cette idole s'est accompagnée d'immolation d'un grand nombre d'animaux sacrifiés.* "(p. 185)

Ad [A] - As shown in JaUT (pp. 10-14), Ja 949 is the earliest of the texts mentioning mhfd since yd^{c}^{3}l/byn was the founder of a dynasty attested at al-^{c}Uqlah. Cf. also below, G.

Ad [B] - Having set her mind that mtll had a ritual meaning and that each royal text reflected such a religious connotation, she distorts the meaning of three words of l.2. She erroneously translates bythn as "le bétyle." This interpretation, however, is not worse than Beeston's translation of byt as "a village with its surrounding land in the Palestinian area" (15). Then, she refuses to acknowledge two other epigraphical facts, viz. the name of the royal palace in Šabwat is šqr of bytn/šqr in ^{c}Inān 10/53 and 56 = Iryani 13/23 and 25, respectively; and in Ja 988/3 and 996/3, the two other references of ṣll prove that the word is a verb. Yet, in spite of that epigraphic material, Pirenne does not indeed shrink from stating without the slightest justification that ṣll of Ja 949/2

is the second name of her "bétyle," viz. "ŠQR ṢLL."

For the verb ṣll, I naturally refer to "hadramoutique moderne ṣll 'séjourner' rattaché à l'assyrien ṣalālu (a) 'sich lagern, ruhen, beschlafen'" (16). Pirenne on the contrary writes: "Peut-être pourrait-on se fonder sur l'hébreu" (p. 173). Why does Pirenne "chercher midi à quatorze heures" and reject the obvious? Instead, she refers to Ar ẓll which she claims has the meaning of "'passer la journée'" (p. 173). This opinion is erroneous: cf. "ẓalla nahārahu yaf^{c}alu kadā he passed his day doing such a thing" and "ẓalla laylahu yaf^{c}alu kadā he passed his night doing such a thing" (17) Thus, the words "la journée" do not belong to the translation of ẓalla.

The conclusion to be drawn from the two preceding divisions of Pirenne's quotation is that there is not much to be said when, confronted with the epigraphical evidence, an author willingly distorts the meaning of a word as well as of byt, of the word ṣll so well illustrated by the other texts, and of the name šqr. At least, such an experience removes any doubt that might still exist that Pirenne will at all cost impose her preconcieved ideas to any text...regardless of anything.

Ad [C] - The statement is an obvious exaggeration. "Tout:" it is quite certain that all the buildings of Šabwat could not have been destroyed; and the "séisme" in fact is a "collapse," whatever the cause, which the text does not specify, might have been.

Ad [D] - The statement is still another consequence of her preconcieved idea that the text must have a ritual meaning. The text, however, has hrgw they killed, which cannot be synonymous of dbḥ to sacrifice, immolate.

E - J. Ryckmans and R.B. Serjeant and Ja 949.

The full text of Ja 949 is given here so that the reader may easily follow the reasonings and argumentations of the following pages.

1 yd^{c}^{3}l/byn/mlk/ḥḍrmt/bn/rbšms/bn/^{3}ḥrr/yhb^{3}r/dsqlb/whrr/hgrhn/šbwt
2 wbr^{3}/bytn/šqr/ṣll/bgndln/mt/tbrw/bn/ṣydmn/whrgw/hms/wšlty/bqrm
3 wtty/wtmnhy/ḥwrw/whmst/w^{c}šry/ṣbym/wtmnwt/^{3}fhd/bgndln/^{3}mwdm

1 Yada^{c}^{3}il Bayyin, king of Ḥaḍramawt, son of Rabbšams of the freemen of Yuhabi^{3}r, him who had transformed and altered the city Šabwat
2 and had rebuilt the palace Šaqar, has sojourned in the fortress when they [the damaged buildings involved] had broken down because of the collapse, and they killed thirty-five bovines
3 and eighty-two young camels and twenty-five gazelles and eight cheetahs

at the fortress ʾAnwadum.

Cf. JaMAR, XIV, p. 80 (and 81, where the reader is reminded that "JaUT already noted: 'Needless to insist upon the fact that neither of the two preceding texts, Ja 949 and Ingrams 1, presents a single lead which would suggest the so-called ritual hunt") and JaUT, p. 49.

1 - J. Ryckmans' translation.

Pirenne's note 24 refers to RycCRA's study of Ja 949; RycCRA's translation of the text (pp. 271-272) reads as follows:

> «Yada'il Bayyin (etc...) qui a fondé et peuplé " la ville (de) Shabwa, a séjourné à (ce) rocher " lorsqu'ils ont interrompu (?) " la chasse " après avoir " tué 30 oryx (*hgrm*), 82 bouquetins (*hwrw* ") 25 gazelles (*sbym*) et 8 guépards (*fhd*) au rocher (de) *'wsdm*».

JaMAR's (XIV, pp. 88-89) study of Ja 949 does not refer to RycCRA's interpretation for the simple reason that it was one of the many questions discarded by drawing of lots. But, Pirenne's reference does not leave me any choice but to deal with RycCRA's interpretation here. — Even at first sight, his prose is unacceptable.

2 - It is immediately obvious that the pericope wbrʾ/bytn/ s̆gr is left out in his translation. — The pericope sqlb/whrr is rendered "a fondé et peuplé," which is copied almost literally from Beeston's original translation. It remains without any explanation, except for note 10 (p. 272): "Ces termes ne doivent pas être pris littéralement." What kind of revelation did Ryckmand receive which would justify such a statement? The second half of the note states that

> " La construction de la phrase - relative explicative complétant les indications d'identité de l'auteur - est à notre connaissance sans autre parallèle en épigraphie sud-arabe. " (p. 272).

So, Ryckmans has to invoke an unattested phrasaeology to support his erroneous theory which he copied from Beeston and which is incorporated in RÉS. The whole pericope dsqlb/whrr - - wbrʾ/ - - /s̆gr is related to rbs̆ms, the king's father, as explained in JaUT, p. 18. The relative pronoun d is unexplicable and unjustifiable if ydʿʾl/byn is considered the subject of that pericope because the asyndesis is so well attested in the al-ʿUqlah texts that it could apply to the verb ʿll; then, d would be useless.

3 - RycCRA's note 11 (p. 272) reads as follows:

> " " *gndln* : ce mot a été généralement traduit «forteresse»; mais comme l'observe Bafaqih (1967: 53), le terme s'applique à un autre rocher inscrit qu'il a découvert, et qui, contrairement au rocher *'wsdm* sur lequel est écrit le texte RÉS 4912, ne porte aucune construction. "

gndln a part of his commentary on Ja 949, his affirmation "le terme s'applique à un autre rocher" is erroneous: he should have written "s'applique aussi à un autre rocher." As it stands, his affirmation means that the word of Ja 949 refers to another boulder, and this is contradicted by the following statement:

> " Au point de vue du contexte, cette identification établit que l'incise relatant des travaux à Shabwa se termine par le mot *fqr*. et que l'action exprimée par la phrase principale du texte (*sll bgndln*) se situe par contre à l'endroit où celui-ci a été gravé (voir déjà Bafaqih 1967: 52-53). " (p. 273).

"Cette identification" = s̆qr as the palace of S̆abwat. - The preceding argumentation is erroneous because the only proof that the action sll did happen at the fortress ʾAnwadum is the presence of the article -n this after gndl, viz. the Sab article -n instead of the Ḥaḍr form -hn (cf. JaUT, p. 27: 21 times -n against 12 times -hn in the texts). — Ryckmans' mention of "BAFAQIH" refers to BeANU which I have never utilized in the past because this publication does not deserve much consideration (19); e.g., the transliteration of Ja 949 (p. 67) contains no less than six errors, and in the facsimile (pl. 15, bottom), "s̆lt" must be corrected to s̆lty (cf. also below, J).

4 - In his note 12 (p. 272), Ryckmans refers to BeSAH who translates tbrw/bn "au sens figuré 'were withheld/frustrated from'." Even at first sight, such an opinion does not make any sense.

BeSAH's reading of 11.2-3 of the text (p. 70) is as follows:

> " *wḍrs/br'* (*Jamme*) *bytn lqr sll bgndln*[364] *mt tbrw bn sydmn whrgw khmst wslty bqrm*[364]*/wtty wtmnhy hwrw*[365] *wkhmst w'lry sbym wtmnwt 'fhd bgndln 'nwdm*. For this I should like to suggest a rendering, "And (the 'king') crenellated (*drs*[366])/built (*br'*) the temple of Shaqr at Jandal with *salil*-stone[367] when they were withheld/frustrated from hunting, and they slew 35 ibex, 82 wild cow, 25 gazelle, and 8 lynx at the Jandal/*jandal* 'NWDM". "

This translation is immediately followed by this commentary:

> " The interpretation would then be that, when the Hunt had failed to take game, the "king" embellished the temple of the Moon God Shaqr with ornamental battlements/or/ good roofing, and they were then successful in hunting and made a bag (which probably was so outstandingly notable as to exceed anything previously known). "

a - The word "drs," which is Beeston's correction of Philby's inaccurate copy, was corrected to brʾ by JaUT; even Bafaqih read brʾ. BeSAH's commentary gives a significative detail which illustrates not only his own attitude but also Beeston's. He writes that

> " Professor Beeston considers however that I go too far in rendering *drs* as "crenellated". " (p. 70).

Serjeant even adds several extravagant flourishes to his "to crenellate" which becomes "to embellish with ornamental battlements" in his commentary (cf. above). On the one hand, what is SeSAH's purpose in publishing Beeston's remark? Nobody knows. At any rate, he simply brushes off that objection without a single word to justify his translation which he bases on the Ar 6th form taḍārasa; cf. his note "» Cf. Lane, Lexicon, taḍāras al-bināʾ, "the building was, became uneven, having in it what resembled aḍrās, teeth"." (p. 110).

Thus, he transforms a static verb into an active one and specifies without any justification that the state of being "uneven" or "irregular" concerns the top of a building rather than the front; cf. the masdar of the 2nd form, taḍrīs "an indentation, or serration" (20). What a waste of time since his "ḏrs" is an erroneous reading suggested by Beeston!

On the other hand, it is also highly significative, but understandable, that Beeston did apparently not mention to Serjeant that his reading of "ḏrs" was erroneous: he must have been very happy seeing his old reading ... being revived. At any rate, I hereby defy publicly both Beeston and Serjeant to prove that brʾ is not the correct reading and that their "ḏrs" is one of what Serjeant calls "alternative readings."

b - The al-ʿUqlah texts prove that gndl is a substantive designating the boulder upon which ʾAnwadum was built. Serjeant's second error is to consider it as a proper name, "Jandal," which should be corrected to "Jandalān."

c - Serjeant's third major error is to place a temple at ʾAnwadum; the texts indicate that ʾAnwadum was a fortress.

d - Serjeant's fourth major error is to consider ṯqr as a temple; it is a well-known fact that ṯqr is the royal palace in Šabwat.

e - Serjeant's fifth major error is to so disturb the grammatical structure of the phrase as to deprive ll.1-2 from any independent verb since the relative pronoun d of l.1 would govern the three verbs sqlb, ḥrr and his "ḏrs" governing bytn/ṯqr/ṣll. The reader of Serjeant's book could not possibly detect or even presume the anomaly of his translation because he carefully omits to reproduce the whole l.1 of the text.

f - The word tbr is constantly found in SA with the meaning of "loss, ruin" and "to destroy, crush, ruin;" cf. also Ar tubūr "perdition; loss, destruction" and tābir "suffering loss, perishing" (21). Obviously enough, Serjeant does not explain at all the reason why an exception should be made in the present text.

g - Neither Serjeant nor Ryckmans explains how the meaning of "to destroy, crush, ruin" taken "au sens figuré" would mean "to be withheld/frustrated," and why the verb should be considered as being used "au sens figuré" in the first place. Moreover, because of a reason which he kept for himself, Ryckmans endorses Serjeant's trend of idea on tbr but adds to it another twist by transforming it into "ont interrompu (?)." How is it possible to translate tbr in such a way? But, such is the ... pity of SA studies conducted by the laudemus invicem club members that what counts first of all and must at all cost be maintained is their preconcieved ideas for the sake of which everything must be bent and distorted and falsified. Finally, Ryckmans adds that

> (1967: 52-54): «revinrent de», interprétation qui ne tient pas compte du fait que la suite du texte place la chasse à l'endroit même où les chasseurs auraient dû «revenir» à l'issue de celle-ci." (end of note 12). "Bafaqih

Ryckmans does not reckon with the fact that, for Bâfaqîh, the first mention of bgndln (1.2) refers to a boulder different from bgndln (1.3); cf. above. Furthermore, Ryckmans' argumentation clearly illustrates his manifest lack of good common sense. When the text says hrgw they killed 150 wild beasts bgndln at the fortress, it is ridiculous to implicitely suggest that all those wild animals would somehow have congregated b at = atop the boulder and would have been killed right there. Obviously enough, good common sense demands that the hunt take place b at = in the vicinity of that boulder.

5 - Ryckmans' note 13 concerns the word ẓydmn and reads as follows:

> " 13 Avec Bafaqih et maintenant B[illegible] (cité par Serjeant 1976: 70), nous estimons que *ẓydmn* se trouve en rapport avec la chasse; il y a probablement une faute du graveur pour *ẓydhn*." (p. 272).

a - The information on "BEESTON (cité par SERJEANT)" is tendencious. Here are the facts. Serjeant writes that "I should like to suggest ... from hunting" (cf. his first quotation above, p. 65). Then he writes (p. 71) the following statements:

> " Of the difficulty over *ẓydmn*, Professor Beeston writes, "I would concur with your rendering of *ẓydmn* as 'hunt', but one has to admit that this poses very difficult problems. There is no reliable evidence in the Semitic languages for a noun-stem *fʿlm*; and I am obliged to fall back on the *pis aller* of supposing an engraver's mistake. This can have happened in one of two ways. The simplest is just that *ẓydmn* is a mistake for *ẓydhn* (which would be the normal *Ḥaḍramī* form)."

b - It always is a cause of amazement for me to see all those authors having no difficulty at all to recourse to "an engraver's mistake" whenever it suits them. The case of Beeston is all the more obvious in that

he himself, in his edition of Philby's copy, had referred ṣydm to the root ṣdm. But, of course, infatuated as he is with the so-called ritual hunt (22), Beeston could not at all resist the temptation of endorsing Serjeant's theory; e.g.,

> " If Beeston's original translation be preferred, and the first two categories of animals immolated be understood as 35 oxen and 82 sheep, these must be regarded as sacrifices connected with the temple. " (p. 71).

Unfortunately, there is no question of themple in the text; the last two categories of wild beasts are ignored, and as stated above, p. 63, the verb is hrg to kill, not dbḥ to sacrifice, immolate.

6 - Ryckmans' note 14 reads as follows:

> " « C'est le sens prégnant que nous donnons à w- dans wḥrgw; littéralement: «et ils ont tué». "

This suggestion is valueless because it is the unescapable deduction from his erroneous, presented as doubtful, translation of tbr as "interrompre (?)."

7 - Ryckmans' expression "30 oryx (bqrm)" is doubly erroneous. The number is clear in the text, ḫms/wšlty thirty-five, and the translation of bqr as "oryx" remains unjustified and is, thus, rejected out offhand. Serjeant writes that "Baqar can mean ibex" (p. 71) or "baqar or cattle mean the ibex.[22] (p. 7) with reference to "Lane, Lexicon" (p. 88). But, LaAEL (p. 234B) indeed writes that "in Egypt these appellations [= baqartu l-waḥši and al-baqaratu al-waḥšīyatu] are applied to the antilope defassa of modern geologists;"cf. also Beeston's statement (23) on "the oryx, which is known to the Arabs of the present day as bqr wḥšy 'wild ox'." These appellations are not real names but only the application of a generic description to individual species because the same LaAEL also writes "baqaru l-waḥši The bull and cow, or bulls and cows, collectively, of the desert; i.e., the wild bull and cow, or bulls and cows" (p. 2929B). Furthermore, since Serjeant is of the opinion that "it is unnecessary however to press the point as to the exact species of the animal which was portrayed" (p. 7), it is impossible to understand why he chose the name of a particular species - as Beeston did - instead of the generic appellation. Moreover, no Ar author, poet or not, can prevail against the simple epigraphical fact that SA bqr always refers to either bovines or cows (cf., e.g., ʾtwrm/wbqrm in Ja 665/26).

8 - Ryckmans translates ḫwrw as "bouquetins" with reference to his note 15 (p. 272) where the reader finds this: "SERJEANT ... traduit 'wild cow' (arabe ḫawar, 'taureau')." Since Ryckmans does not endorse Serjeant's translation, the parenthesis "(arabe ḫawar, 'taureau')" must be his understanding of

Serjeant's argument or his own explanation of Serjean's reasoning. In either case, it is wrong. Serjeant does not refer to Ar "ḫawar, 'taureau'." Indeed, Serjeant's testimony is clear: "

> I take ḫwrw in the sense of the Arabic ḫawrā' as in al-Ānisī's[sic] al-rashā al-aḫwarī applied to a gazelle and a young girl, or to a wild cow. " (p. 71) and
>
> " rashā aḫwarī, gazelle, girl, wild cow, 71. " (p. 126A).

But, Ar ḫawrâʾ is the feminine of ʾaḫwaru (24);
Ar rašan means "gazelle" (25); and
ʾaḫwaru means "having eyes characterized by the quality termed ḫawar" and is applied to man as well as to woman (24); and ḫawar means "intense whiteness of the white of the eye and intense blackness of the black thereof ... or the blackness of the whole [of what appears] of the eye, as if in the eyes of gazelles ... and of bull and cows" (26).

Here again, Serjeant's argumenting method is revolting: a feminine adjective describing a physical characteristic of a gazelle is taken as the meaning of a SA noun, and he takes for granted - of course - that SA ḫwrw is feminine. To JaUT's (p.50) commentary on ḫwrw, add the following information: cf. Ar ḫwâr "chameau dans sa première année" and ḥʾwêyer "jeune chameau" in C. Denizeau, Dictionnaire des parlers arabes de Syrie, Liban et Palestine (Supplément au dictionnaire arabe-français de A. Barthélemy), Paris, 1960, p. 128.

F - J.-F. Breton et al. and Ja 949.

The preceding study of Pirenne's, Beeston's, Ryckmans' and Serjeant's theories on Ja 949 illustrates also the argumenting method of J.-F. Breton (27) in his summary of "RES 4913" which should be corrected to RÉS 4912 = Ja 949, where he reads "Sydnm" (p. 184) which should be corrected to ṣydmn.

> According to Breton, the text would mean that "Yadaʿʾil Bayyin, roi de Ḥaḍramawt 'reconstruit le temple en pierre, pose un toit et un dallage dans Šqr lors de leur (le temple et le château) effondrement à cause d'un tremblement de terre'" (p. 184).

The grammatical sequence and the meaning of the first third of 1.2 is all mixed up, the expression bgndln is left out, and there is no mention of a "temple" in the text. The author concludes his summary by adding that "et fait un sacrifice à cette occasion." Here again, the author repeats the major error of hrg to kill = dbḥ to sacrifice. Finally, the author adds this ominous statement: "De nombreux indices ne font que confirmer cette lecture (cf. supra)." "Cette lecture" undoubtedly refers to his so-called translation and interpretation of the text. But, the only element of his translation which might be confirmed by "de nombreux indices" is his mention of an earthquake the effects of which are described on pp.173-174.

However, one statement, viz. "le bâtiment B - au moins - a résisté à un terrible tremblement de terre dont les traces sont encore parfaitement visibles" (p. 173), raises a disturbing question.

These effects are visible in building A, not in building B. But, this building A is much sturdier and robuster than building B, and the two buildings are slightly imbricated. How is it possible that the weakest building B would have withstood unshaken and undisturbed "un violent [my underlining] tremblement de terre" while the strongest contiguous building A would have greatly suffered even to the extent that "sous le choc, certaines bases de piliers du portique ont été brisées en deux et se sont redressées de part et d'autre des poteaux, ces derniers poinçonnant" (pp. 173-174)?

The building complex analyzed in the paper is presented as a "monument vraisemblement comme le château royal" "(p. 163) or "'château royal'" (p. 163) or simply "'château'" (pl. 1) or "château" (p. 163 and pl. 2) or "palais" (pp. 174-177). In a more detailed manner, it is said that "cet ensemble monumental peut, à juste titre semble-t-il, être assimilé au château royal de Šqr des inscriptions" (p. 164); "il convient de replacer le noyau principal de la 'demeure royale', les appartements princiers du château" (p. 171) in building A; summarily, "aucun argument ne permet .. d'affirmer ou d'infirmer l'identification proposée... Provisoirement, tenons donc pour plausible cette identification du château Šqr" (p. 178).

Yet, the main difficulty against the preceding identification of the building complex with the royal palace may be presented as follows:

(1) it lacks of any special individual defense; the buildings do not even look like strongholds at all with two open undefended entries, 1.50 m wide each (p. 172);

(2) its vulnerability is further enhanced by the proximity of a gate (about 12 m inbetween) and of the western wall (as close as 7 m);

(3) the gate, which is characterized as being "la porte principale" (p. 164), appears very weak if compared with the main gate of Sumhuram (28) and even weaker than the South Gate of Timnaʿ.

Summarily, it is difficult to see in that complex the most important building of the city, that which should be most secured by strong defenses and certainly impervious to even the bold attack of any raid. - Note also that on p. 189, the title of J. Pirenne's paper, already incomplete in note 31, is further abbreviated and partially distorted in note 36.

G - Pirenne's basic argument.

Pirenne's decisive argument in her theory apparently is this:

"Mais ce qui n'a pas été signalé et qui est primordial, c'est qu'un bassin arrondi flanque le bastion, et que son bord supérieur arrive exactement au bas du mur du « fortin », comme si l'eau avait pu couler de dessous celui-ci dans le bassin. Et ce bassin-là est ancien." (p. 184).

That the fact referred to by her "n'a pas été signalé" is erroneous, JaUT (p. 2) states that "the south half of the east side of the boulder serves as the back wall to an enclosure."

What is so primordial about such a fact? Is it not normal for a builder to use one side of the boulder to build an enclosure if he wanted to build it there? — Of course, that enclosure is ancient. But, the most crucial question is: how old is it? Here again, the most fundamental fact which Pirenne ignores is twofold. (a) The enclosure wall was built at a later date with stones from the mḫfd because they are reused in its masonry. (b) As stated by JaUT (p. 6), "most of the text [= Ja 919] was covered by the west extremity of the north wall of the enclosure" and

"I had to remove enough cement from the narrow interstice between the northern edge of the second boulder and the south side of the main boulder to make sure that I had the entire text of Ja 962."

I simply cannot understand how Pirenne could ignore such an important information.

H - Pirenne's conclusion.

After wasting almost two pages explaining her imaginative theory, Pirenne shouts her victory-cry by stating that: "Nous aurions donc [in these texts] ... des attestations de ... pratiques magico-religieuses pour obtenir la venue de la pluie" (p. 173) or

"visites de trois rois venus, selon mon interprétation, pour accomplir une marche rituelle et faire une incantation (magique) de noms, en vue de faire venir l'eau." (p. 184).

1 - To see in the mḫfd of ʾAnwadum a "condensateur de rosée" is a wild nonsense. Of course, the dew, when it falls, moistens everything on the ground and drops of water may normally result from the dew accumulation; to handle in a chilly and windy predawn objects left outside during the night may be a downright glacial experience. As it is well-known, that dew is the only humidity many wild animals can find. But, in spite of the examples given by Pirenne (p. 164) (29), there is a world of difference between the preceding natural phenomenon, which may occasionally be impressive, and the feeding of a basin with water because the dew evaporates as soon as the sun starts shining and, if by an extraordinary chance, the dew had formed a thin trickle of water (I have never read or heard of such a case), that too would never last very long in the day's heat. If such is the case, who would waste his time building a "condensateur de rosée" which

would practically collect nothing at all during the period of the year when he may expect the dew?

Only a religious fanatic would see in the texts some "pratiques magico-religieuses." Furthermore, she completely forgets that her pp. 159-220 deal with "Rosée et Arabie du Sud" and her p. 175 is introduced by "Recupérateurs de rosée en Arabie du Sud;" and not at all of ... rain. Moreover, JaUT states (p. 13) that "the small enclosure ... is posterior to the historical period during which the al-ʿUqlah texts were engraved: it was built with stones that fell down from the top building, and the west extremity of its north wall was still, at the time of my arrival, covering most of Ja 919."

The preceding archaeological fact also provides another example of Pirenne's distorting argumenting period. On the one hand, she endorses my previous testimony as "cette observation utile" (p. 184). On the other hand, she speaks of "son réaménagement" ... without the slightest proof. But, she needs that imaginary concoction very badly to state that

> " *Il reste que la présence du bassin arrondi, qui était certainement en connexion avec la structure, avant son réaménagement, corrobore l'hypothèse d'une nature hydraulique du mahfid, puisque tel est le terme technique qui la désignait, lors de son usage initial.*" (p. 185).

2 - The preceding statement is another proof that her theory rests upon her distortion of the material, both epigraphical and archaeological. Her chronology rests solely on this: "là [= in Ja 949 of yd^{c}^{3}l/byn] il n'est plus parlé de mahfid mais de ġandil" (p. 185).

The truth of the matter is that mḥfd is still attested in Ja 957/4, another text engraved during the reign of the same king yd^{c}^{3}l/byn. In fact, the word mḥfd is also mentioned in the texts of the successive reigns of yd^{c}^{3}l/byn's two sons, viz. Ja 988/3-4 of ^{3}lrym/ydm, and Ja 996/3 of yd^{c}^{3}b/ẖyln. This prove that the mḥfd was built a f t e r ġydmn the collapse mentioned in Ja 949/2, and those three successive reigns are posterior to that of ^{3}l^{c}d/yl^{1} as demonstrated by JaUT, pp. 13-14. That the mḥfd was built by yd^{c}^{3}l/byn is proven by the fact that, during the reign of his predecessor ^{3}l^{c}d/yl^{1}, not a single text has the word mḥfd; all these texts have ġndl (Ja 921/3, 923/3-4, 925/6, 926/5 and 928/6-7). It is, thus evident that Pirenne's theory, viz. "al-'Uqla était, dans son état originel, un *mahfid* " (p. 196) and

> "*Il en résulterait que le mahfid aurait été endommagé aussi par le séisme (dont on retrouve les traces évidentes à Šabwa, par la fouille) ou bien que le changement du climat le rendait inopérant et qu'on a abandonné son usage hydraulique.*" (p.185)

is precisely the opposite shown by the texts: le mḥfd did NOT exist at the time of

the disaster which caused ġydmn the collapse of the buildings. Obviously enough, there cannot be any "réaménagement" of a thing ... that did not exist.

3 - Since "the small enclosure" was built up with stones from the building located above the rock, it is obvious that it was built after the reigns of those three kings mentioned in the texts, and after the upper building had been abandoned because it was no longer in use. JaUT (p.3) suggests that the small enclosure was a jail because of "Ar ʿuqlah means 'bond, shacke', and 'jail'" p. 3) on the basis of LaAEL, FrLAL, etc. Pirenne's verdict is that "du point de vue de la vraisemblance archeologique, l'hypothese se passe de commentaire" (p. 184) apparently because, blinded by her theory, she is unable to find any.

The first meaning of Ar ʿuqlah given above does obviously not fit her dream, so she had to find something more appropriate. She writes that" al-Hamdānī l'interprétait ainsi : « espace où, en quelque point qu'on déblaie le sable, on est assuré de trouver de l'eau [21]. » and her note 21 reads as follows:

"21. al-Hamdāni, selon Forrer, cité dans Dozy, *Supplément aux dictionnaires arabes.* "

But, the texts do not speak of digging into the sand; they speak of a building atop a boulder. My "commentaire" on her statement is fortright: her statement is another clear example showing how Pirenne voluntarily distorts the written words of the authors. I say voluntarily because the writings of those authors are so evident, so simple that nobody in his right mind can misunderstand them. The reason of Pirenne's imposture also is simple and evident: she wanted to back up her theory with al-Hamdânî's authority. In fact, the chain of authors suggested by her statement is grossly inaccurate since Hamdânî is not involved, as shown by the following schema:

Pirenne	In fact
al-Hamdânî	Carette
↓	↓
Dozy	Dozy
↓	↓
Forrer	Forrer

The facts are as follows. L. Forrer mentions this: "al-^{3}Uqla - der ʿUqla[15]) der Ḥaṭārir" (30). That is all that al-Hamdânî says. After giving E. Glaser's identification of that ʿUqlah "westlich des Ğ. Barat" (south of Saʿdat in North-Yemen), Forrer's note 15 (31) adds this information: "Zur Bedeutung: Dozy (Supplément aux dictionnaires arabes): Espace où, en quelque point qu'on déblaye le sable, on est assuré de trouver de l'eau." And R. Dozy (32) refers his translation to "Carette, Géo. 124," viz. Carette, "Recherches sur la géographie et le commerce de l'Algérie méridionale. Paris, 1844 (t. II de l'Exploration de l'Algérie)" (33). Therefore, Pirenne prefers the meaning of the Algerian Ar dialect to the first meaning of classical Ar without any justification.

4 - As a consequence of her erroneous historical sequence, Pirenne states this:

"Le roi y a installé une idole, dont on a retrouvé le podium. [A] La connexion avec le bassin a été abandonnée et cachée sous le cimentage [B] [C] refait. Les bassins ne devaient plus servir qu'en cas de pluie." (p. 185)

Ad [A] - The mention of an idol comes from her erroneous translation of byt (cf. above, p. 52). Furthermore, what she calls a "podium," who revealed to her that it was for an idol as if it were impossible that that block could not have supported the king's statue or bust.

Ad [B] - Her photograph of pl. XXII does not show any trace of masonry on the side of the boulder above the enclosure; in fact, there is none and the so-called connection did not exist and, therefore, could not have been hidden by cement.

Ad [C] - Were it the case, the enclosure was on the wrong side of the boulder because the rock partly overhangs the enclosure. Were it destined to catch rain, it would have been very easy to rebuild it on another side of the boulder.

Moreover, JaUT states that "these facts [cf. my quotation above] which Philby [(34)] could not have missed, did not refrain him from stating that the enclosure 'was presumably intended for watering the camels of the garrison'. [] it is not easy to visualize camels drinking across the high wall of the enclosure" (p. 13). According to B. Doe (35), "the walls" of the enclosure are "about 1.5 m high" (p. 23). This information gives the medium height and in fact is very deceptive because the reader may think that the height is between, e.g., 1.2 and 1.7 m, and he would have been misled into error because the two extreme points of the wall are much higher. As stated by JaUT, the day of my visit to al-ʿUqlah, on Sunday, January 28, 1962 (p. 1), "the wall" of the enclosure was "from about 3 to 7 feet high above the present level on the north and south, respectively" (p. 2), about 91 cm and 2.13 m. Such an enclosure was not "intended for watering the camels of the garrison," as Philby suggested, because it was built after the reigns of the Ḥaḍr kings who used the boulder. Nor was it intended for watering any camel or any animal because those beasts would have had necks longer than those of giraffes and as flexible as elephants' trunks. Nor was it intended for the human use of drinking water because it would have been against the natives' good common sense to pour their drinking water into an open-air enclosure and to drink it from that basin. Moreover, Doe who does not refer to the publications of Bāfaqīh and Pirenne, who both worked at al-ʿUqlah, also is of the opinion that "at some later

time, Anwad seems to have been used as a water catchment" (p. 23), an idea that comes from Pirenne; thus after the time of the Ḥaḍr kings mentioned in the texts. It is unfortunate that Doe does not indicate either the century he had in mind when he wrote the expression "at some later time;" or give the reason or reasons which might justify his interpretation; or indicate who were the people who used that enclosure as "a water catchment;" or what kind of water those people did in fact catch; or, finally, the reason or reasons why those people would have built "a water catchment" in the middle of nowhere?

The truth of the matter is that Pirenne's statement to the effect that

" le monument que nous avons fouillé n'était plus un *maḥfid*, et ne nous donne qu'une idée inadéquate de ce que cela pouvait être." (p. 185) is the best proof that, as stated above (p. 54), she "invents what she would like to see." The archaeological interpretation of any monument rests solely on the careful evaluation of all items found during the excavation; therefore, to claim that the monument had a function of which no trace at all remains in to indulge in an irrational thinking contrary to the fundamental principle of archaeology.

5 - Pirenne's final touch of her study is the following phrase reproduced above, p. 72, center. Of course, there is a relation between the enclosure and the fortress; it is the boulder itself which backs the first and supports the second. But, her statement is nothing but an obvious vicious circle because what she considers as her final reason, "puisque ... initial," is precisely what she should have proven. But, instead of proving it, she uses it as her jumping-off place to, or her reason for, her understanding, which is a misrepresentation of the material relation between the fortress and the enclosure.

6 - Another characteristic of Pirenne's method is, as stated above (p. 54), that she "closes her eyes to what she does not wish to see." There is one text of the al-ʿUqlah collection which she was forced to ignore in order to formulate her dreamy theory. Yet, that text, she saw it herself during her work at al-ʿUqlah; cf. her own testimony: "en copiant les inscriptions nombreuses (et déjà publiées) qui couvrent le rocher" (p. 184). Therefore, Pirenne's attitude is deliberate. The text, Ja 973, is engraved on the west side of the boulder ʾAnwadum and reads as follows:

ʾdnm/bn/kb	1	ʾAdnum, son of Ku-
yrm/ḥwl/mh	2	bayrum, was in charge of the
yhn/bʾnwd[m]	3	water-supply in ʾAnwad[um] (cf. JaUT, p. 55).

What could have been ʾAdnum's job had Pirenne's dream been a reality? To scrape

the wall of the rock so as to collect every bit of dew? To distribute tickets or tokens to all those who had not taken their water-supply along with them? To be some kind of traffic cop to make sure that everything went smoothly? What else could he have been doing? In fact, ʾAḏnum was in charge of the water detail,viz. it was his job to provide the royal party with the necessary water-supply for the duration of the trip.

There remains the question to know from where the water was brought to al-ʿUqlah. JaUT states that "there is no sign of any well either in the neighborhood or between al-ʿUqlah and an-Nasr" (p. 3). Pirenne endorses this testimony(p.184) but fails to refer to BaANU whose map (cf. also paragraph 13) locates "al-ʿābār" the wells about 1.5 klm north of ʾAnwadum. However, there is no proof that those wells date from the pre-Islamic era and more specifically from the period of the use of ʾAnwadum by the Ḥaḍr, and it remains doubtful whether archaeology will ever be able to give them a date. On the one hand, in the hypothesis that they were dug before the building of ʾAnwadum, it would be manifestly ridiculous for the royal founder of ʾAnwadum to build a "condensateur de rosée," as Pirenne claims it to be, which would be practically useless, since there was plenty of water not too faraway. Then, ʾAḏnum's job was to bring water from those wells for the royal party. On the other hand, if those wells had not yet been dug before the use of ʾAnwadum by the Ḥaḍr kings, it would still be no less ridiculous for a king to erect that kind of useless building dreamed by Pirenne. Then, it was ʾAḏnum's duty to bring water from Ṣabwat.

I - The site of al-ʿUqlah.

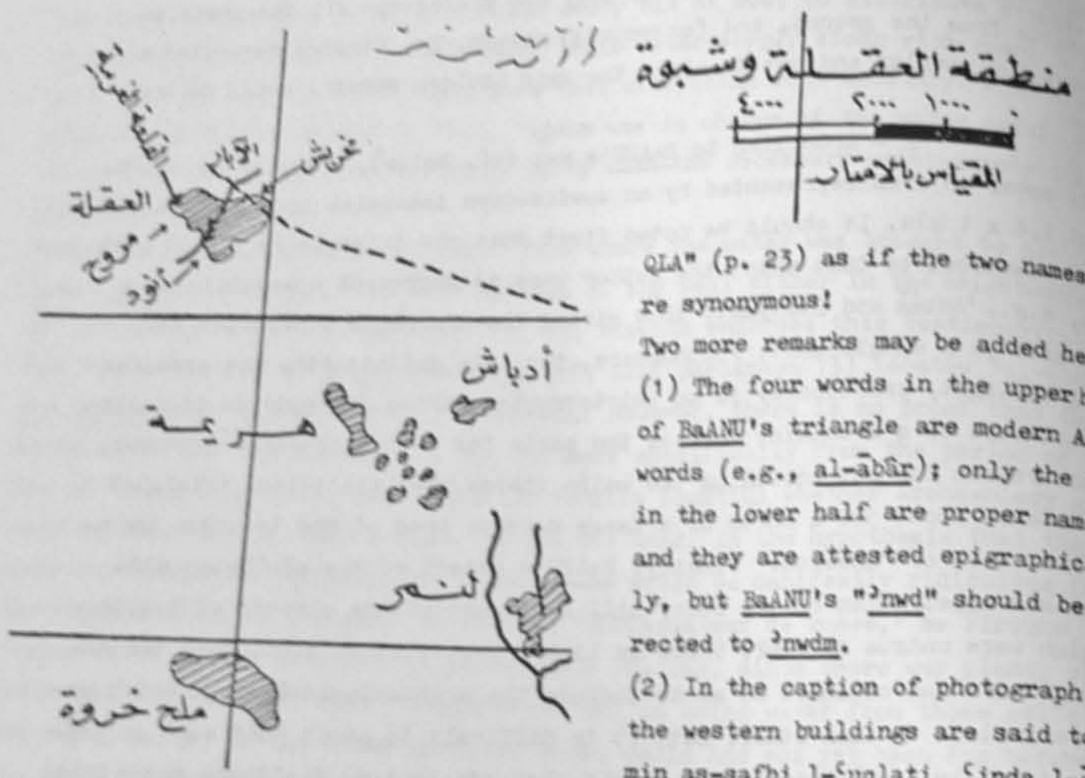
1 - The complex of al-ʿUqlah is a cluster of four imposing boulders in the middle of a wide plain. There are other small outcrops in the surrounding area as well as two hills nearby, but, according to the explicit testimony of my native travel companion, all of them were nameless and not understood under the name of al-ʿUqlah. Among my travel companions was the commanding officer of the military post of an-Nasr and two of his soldiers. These three military men were Arabs and, needless to say, knew well the topography of the region they patrolled every day in order to keep track of all travelers, isolated or in groups, regardless of their destination or origin. When I was there, Sunday, January 28, 1962, the situation was so tense that Mr. Richard Etridge, the political officer at al-ʿAbr, ordered one of the soldiers to be at my side constantly and four others to stand guard around the complex. Since it had taken us three hours to cover the 69 miles from al-ʿAbr to al-ʿUqlah, including 27 minutes from an-Nasr to

al-ʿUqlah, I worked as fast as possible to copy and photograph all the inscriptions readable from the ground, and for security reason, Mr. Etridge requested me not to leave the complex and not to climb the main boulder where I would be a perfect target.

2 - According to BaANU's map (cf. below), the site of al-ʿUqlah would cover an area represented by an upside-down isosceles triangle measuring about 1.6 x 1 klm. It should be noted first that the triangle is filled with a hatching identical to that which the author uses to represent a mountain or a hill; cf., e.g., ʾAdbāš and Huraymat, thus giving the erroneous impression that the whole site is a single unit. Furthermore, the line delimitating the area,far from being straight, practically is an uninterrupted series of bends as if following the contours of a mountain. What is the basis for such a tracing? Moreover, what is the reason for not including the wells inside that fictitious triangle? They are located about 500 m north of a large concave bend of the triangle and about 1.5 klm north of ʾAnwadum. Finally, BaANU's extent of the al-ʿUqlan site as well as his appellation of the rocky hill northwest of the cluster of boulders as al-ʿUqlah were untrue in 1962. There is little doubt, in my mind, that the name of the cluster was extended so as to include the archaeological sites which were relatively close to each other. But, it is difficult to admit that such an initiative be taken by local bedouins. I should also add that my fieldbook notes this: the main boulder ʾAnwadum"se trouve exactement au sud du plus grand des deux pics avoisinants." Yet, BaANU's map places his hill "al-ʿUqlah" 1 klm northwest of ʾAnwadum. Furthermore, Ryckmans speaks of

> au pied d'un cône volcanique de 700 à 800 pieds (Philby 1939: 314) "un piton rocheux appelé al-'Uqla (Bafaqih 1967: carte en fin de volume; Jamme 1963: 2, limite par erreur cette appellation au bloc lui-même et à ceux qui l'entourent)" (p. 271).

Ryckmans inaccurately uses the word "piton" to characterize a more or less cubic boulder with a flat top. It is interesting to note that it is the very same word "piton" which is also used by C. Robin when referring to the boulder (36). "Piton" is by definition the peak of a high mountain! Furthermore, Philby's text describes the rocky elevation north of ʾAnwadum as "the hill" (p. 234, ll. 1 and 1) and "the main hill" (same page, last line). Yet, although referring exclusively to Philby's book, that "hill" becomes "un cône volcanique" in Ryckmans' jargon.The balance of Ryckmans' statement is taken care of in the preceding introduction. Moreover, Doe also calls the main hill "Jabal al-ʿUqlah (23), a name which did not exist in 1962. Doe goes even as far as to entitle his paper "ANWAD, JABAL AL-ʿU-



QLA" (p. 23) as if the two names were synonymous!

Two more remarks may be added here.

(1) The four words in the upper half of BaANU's triangle are modern Ar words (e.g., al-ābār); only the two in the lower half are proper names and they are attested epigraphically, but BaANU's "ʾnwd" should be corrected to ʾnwdm.

(2) In the caption of photograph 12, the western buildings are said to be min as-safḥi l-ʿuqlati ʿinda l-ābāri l-qadīmati at the foot of al-ʿuqlat near the ancient wells. Yet, those buildings, under the name of al-qalʿat al-ġarbīyat the western fortress, are located on the map about 700 m to the southwest of those ancient wells!

J - The inscriptions of al-ʿUqlah and vicinity.

1 - It is well-known that JaUT published the inscriptions which I recorded at al-ʿUqlah, viz. 97 entries, Ja 910 = Uq 1 to Ja 1006 = Uq 97.

2 - Bāfaqīh's epigraphical work.

a - General remarks.

(1) Beside BaANU's six errors in the transliteration of Ja 949, there are 42 (forty-two) more in his transcription of the other texts, and 1.4 of Ja 921, 1.7 of Ja 923 and 1.6 of Ja 926 are not indicated as such.

(2) The worst error is Bāfaqīh's substitution of "byn" instead of ġyln in Ja 997/4; cf. the photograph of the text in JaUT, pl. IID.

(3) Bāfaqīh's transliteration of the texts in Ar letters was not made from his own copies of the originals; it is a mere transliteration of JaUT's transcriptions in SA letters. Ja 936/1 proves the case.

In JaUT (p. 45), the letter r was forgotten in JaUT's copy although the letter is accounted for in the translation. The very same letter r also is missing in BaANU's text on p. 64.

(4) BaANU indicates that Ja 961 should be read as a dextrograde text (p. 72), instead of senestrograde as in JaUT (p. 56). Indeed, in Ja 916, 946, 974, 975 and 1001, the word divider is placed after the name or the initial. However, the ductus is more important than the place of the word divider; therefore, JaUT's interpretation is justified by the senestrograde ductus of two letters, ly, contrasting with only one dextrograde n.

(5) BaANU brought to light eight more texts which he found in three different locations in the vicinity of al-ʿUqlah, viz. BaANU 1-6 (pp. 77-81). Those texts are ignored by Doe (p. 24) who limits himself to the number of "97," which is JaUT's number. Indeed, he considers only "the inscriptions on the main boulder ..., and on the nearby boulders." Therefore, Doe ignores J. Ryckmans' mention of one (37) of those eight new texts, BaANU 1 (cf. below).

(6) It is deplorable that, when she was working at al-ʿUqlah, Pirenne did not gather a good photographic documentation of all those new inscriptions to supplement BaANU's poor publication which contains only three more than mediocre photographs.

b - BaANU 1: BaANU places the text (cf. photograph 7) on the northern section of a boulder (cf. photographs 6 and 9) at about 1.80 m above ground level (p. 79). The text may be read as follows:

1 yd[ʿ]l/byn/mlk/ḥḍrmt/
2 bn/r[ʾ]b[ʾ]l/syr/[ʾ]d/
3 gndln/mrwḥ/hslqb

1 Yada[ʿ]il Bayyin, king of Ḥaḍramawt,
2 son of Ra[ʾ]ab[ʾ]il, went to
3 the boulder Marwaḥ in order to give titles.

For the transliteration, cf. BaANU, p. 59, where, however, bn is placed at the end of l.1. The correct contents of the lines, however, are maintained in the translation (p. 77). J. Ryckmans' transliteration erroneously has "ḥ" instead of s in syr, and fails to refer to BaANU's p.59 or 77; his erroneous "ḥr" may be explained by his obsession of one of his favorite thesis, the so-called ritual hunt. - syr and hslqb: already five and four times, respectively, in the al-ʿUqlah collection (cf. JaUT, p. 72A).

c - BaANU 2: BaANU found the text halfway between the preceding text and the ground floor (p. 79). There unfortunately is neither photograph nor facsimile in the edition. — The editor reads "mrbhm" but adds "possibly mrbḫm (see what is above)." The first reading gives a very difficult Ḥaḍr name; and "what is above" is mrbḫm in Ja 991/1 = BaANU, p. 74. The name is mrbḫm Marbaḫum.

d - BaANU 3: according to BaANU (p. 79), this name is at the bottom of the same side of the mrwḥ boulder and somewhat to the left of the preceding name. — The editor reads "mtnm" which becomes in Ar "mtnm ʾaw mtynm;" the absence of vowels prevents the reader to know precisely how the editor vocalizes the two names. — mtnm Matnum. - The root mtn is attested in the composite place name qrʿmtn of CIH 342/8-9; cf. also the Saf personal name mtn, e.g., in CIS 4991.

e - BaANU 4: BaANU declares that this name is well engraved and found "ʿalá n-nâḥîyati l-ḥayawîyati li-ṣaḫrati marwaḥ" (p. 79). I suggest correcting الحيوية, which makes no sense, to الجنوبية; thus, the phrase means on the southern side [of the hill which he calls "Jabal al-ʿUqlah"] toward the boulder marwaḥ. — mʿdkrb is read by the editor who renders it in Ar as "mʿdykrb." The "y" is incorrect no matter how the editor analyzes the name, either "mʿd + ykrb" or "mʿdy + krb." It is Maʿadkarib. — The editor starts his commentary as follows: "haḏâ n-naqš(2)," and note 2 refers to photograph 14. But this photograph represents the graffiti BaANU 5. The balance of the commentary (pp. 77-78) mentions the existence of letters obliterated below the name itself, graffiti nearby, and more writing on top of the same hill.

f - BaANU 5: the edition of the texts (p. 80) does not give any detail of any kind on the inscriptions; the reader has to look through the photographs to find that they are reproduced on photograph 14. The caption of that photograph indicates that the graffiti, maḫrabaš, are found "min šamâli l-ʿuqlati." However, they are located east, not "north," of "al-ʿuqlat" on his map. — The editor reads the texts as follows:

ل س ب ه ى م ز ز ع = عز يذم يهبل (٥) (The hand corrections are by the author)

م ح ع ص = م ح ع ص (→)

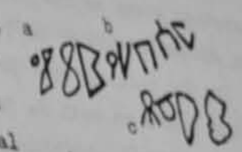
The editor's reading of the texts indicates that the photograph is printed upside-down in the booklet. Although the poor quality of the photograph does not make it difficult to determine which side is up, that the photograph is printed upside-

down is accepted because the editor saw the inscriptions. Furthermore, he indulges in a strange novelty: he transliterates the upper line from right to left (since there is no arrow to point to the other direction) but he translates the line reading the letters in the opposite direction, viz. from left to right:

← "ʿ z z m m y h b s l" (his transliteration), but

→ "ʿzyzm yhbsl" (his translation).

It should be noted that the only two letters the ductus of which indicates the direction of the line, his "l" and m, are dextrogrdae although his decipherment is senestrograde. Moreover, the editor considers the upper line as a single line composed of two names although both the palaeography and the letter height are different in each case. In addition, the ductus of the two z's would fit better in a dextrograde line. Finally, the editor's decipherment of the initial letter of the upper line and of the two central letters of the lower line is inaccurate. If the photograph is to be read as it is printed, text a is senestrograde but texts b and c are upside-down and, seen from the engraver's view point, dextrograde. If the photograph is printed upside-down as read by the editor, the three texts are dextrograde. Regardless of the actual position of the texts, their reading and interpretation remain the same.



(my tracing of BaANU, photograph 14 upside-down)

Text a: in the upper left corner: → ʿzzm ʿAzîzum. - Cf., e.g., Ja 2922 f/1.

Text b: immediately to the right of the preceding name: → yhbsr Yuhabsir. - Cf. the Sab personal name bsr, e.g., in Ja 2194m.

Text c: below the preceding name: → ṣdym Ṣudaym. - Cf. the Sab family/clan name ṣddm in RÉS 4761/1.

g - BaANU 6 (pp. 80-81) is a boustrophedon engraved on a stone the surface of which is still rough, found in the western building atop his "Jabal al-ʿUqlah;" cf. photograph 10 which is very poor and extremely difficult to read. — The editor's reading is as follows:

(٦) ى د ع إ ل / ب ى ن / م ل ك / ح ض

... م / ت م ر

= يدع إل بيّن ملك حضرموت(١)

(١) راجع اللوحات (شكل رقم ٦)

But, the photograph 6 represents the boulder mrwḥ upon which BaANU 1 is engraved. According to its caption, photograph 10 shows the rock bearing the main text of

the western building. The two kings called yaᶜ₃il/byn/mlk/ḥḍrmwt of BaANU 1 and 6 are not identical because of the boustrophedon ductus of the letter text. — The photograph does not show the extreme left letter of both ll.1 and 2; mt of l.2 is below /ḥ of l.1: the next word divider is below the left vertical staff of k of mlk (l.1); then follows m below the space between l and k of mlk (l.1); and below the space between m and l of the same mlk there is a V occupying the upper half of the letter height; thus t. To the right of that V and in the vertical axis of both m of the same mlk and the word divider to the right of mlk, there is what looks like the upper section of two l's. Then, the verb mᵢtllᵢ could be read and that verb is followed by the verb ṣll in the two royal texts Ja 988/3-4 and 996/3-4. — The title of the royal text should normally be followed by the mention of the king's father which, in the present case, would be another Ḥaḍr king. Such an hypothesis would demand that the left-hand section of the inscription be upon another stone or the stone itself if it has been broken off. Furthermore, the reading of the remainder of 1.2 and the whole l.3 is not possible on the photograph. On the basis of the preceding remarks, an hypothetical reading of the inscription based on both Ja 988 and 996 may be presented as follows:

1 ← [..../nb/tmrḍ]ḥ/klm/nyb/lᵓᶜdy

2 → [./.../mlk/ḥḍr]mt/mᵢtllᵢ[/wṣll ...

1 Yadaᶜ₃il Bayyin, king of Ḥa[ḍramawt, son of ....]

2 [. ..., king of Ḥaḍra]mawt, sojourned day [and night ...

BaANU's publication of the new epigraphical material discovered in the vicinity of al-ᶜUqlah by the author himself makes it necessary for a competent scholar to go back to that isolated place, since Pirenne did not care about those texts, completely absorbed, as she was, by the chase of her own dreamy thoughts, in order to gather good photographs of the texts, the museographical information on those inscriptions and their accurate locations.

Notes

(1) Cf. JaMAR, II, pp. 86-88 and tracing on pl. 6, bottom.

(2) Cf. her book La maîtrise de l'eau en Arabie du Sud antique. Six types de monuments techniques, Paris, 1977, pp. 47-48.

(3) Cf. La paléographie sud-arabe de J. Pirenne, Washington, 1957.

(4) Cf., e.g., JaMAR, IV, pp. 186-194.

(5) Cf. l.c., IX, p. 103. (6) Cf. JSS, 27 (1982), pp. 101-108.

(7) Cf. JaMAR, XIV, p. 82. (8) Cf. l.c., p. 79. (9) London, 1946.

(10) Cf. PSAS, 12 (1982), pp. 7-13.

(11) Cf. M. Amin, Pilgrimage to Mecca, London, 1978, p. 21.

(12) Cf. my review of this dictionary, cf. JaMAR, XIV, pp. 62-269, and especially pp. 78-132.

(13) Cf. l.c., p. 80. (14) Cf. l.c., pp. 100 and 120-121.

(15) Cf. l.c., pp. 80 and 105. (16) Cf. Le Muséon, 69 (1956), p. 101.

(17) Cf. LaAEL, p. 1914B.

(18) Cf. also A.S. Tritton's objection based on a secondary aspect of the action dbḥ in BSOAS, 10 (1940), pp. 514-515.

(19) After listing BaANU in his note 362 (p. 110), SeSAH writes that

> "This does not seem to be a publication of much value unless, possibly, it has alternative readings."

It may safely be said that Serjeant's knowledge of Ar does not authorize him to judge the possibility of "alternative readings" in SA.

(20) Cf. LaAEL, p. 1785B. (21) Cf. l.c., p. 330C.

(22) Cf. JaMAR, XIV, pp. 80-89. (23) Cf. Le Muséon, 61 (1948), p. 190 and note 19.

(24) Cf. LaAEL, p. 666C. (25) Cf. DoSDA, I, p. 532A.

(26) Cf. LaAEL, p. 666A. (27) Cf. Raydān, 4 [1981] 1982, p. 184.

(28) Cf. F.P. Albright, The American Archaeological Expedition in Dhofar, Oman, 1952-1953, Washington, 1982, pp. 15-19 and pl. 4-5.

(29) In her reference (p. 164, note 4) to Cheesman's book entitled In Unknown Arabia, correct "B," Cheesman's initial, to "R.E.," and "p. 235" to "pp. 235-236."

(30) Cf. Südarabien nach al-Hamdānī's 'Beschreibung der arabischen Halbinsel', Leipzig, 1942, p. 116. (31) Cf. l.c., pp. 16-17.

(32) Cf. DoSDA, II, p. 155B. (33) Cf. l.c., I, p. XXA.

(34) Cf. Sheba's Daughters, London, 1939, p. 315.

(35) Cf. PSAS, 12 (1982), pp. 23-25. In his note 1 (p. 25), Doe claims that Philby's book deals with al-ᶜUqlah in pp. "313-9." Correct "9" to "6."

(36) Cf. PSAS, 16 (1986), p. 181. (37) Cf. Le Muséon, 87 (1974), p. 521.

V - Remarks on V.A. Clark's publication of seven new Safaitic inscriptions.

A - Introduction.

1 - The first two limestones with one text each come from the vicinity of Sakākah, a city located in the northwestern section of Saudi Arabia and whose coordinates are "29°59'N - 40°12'E" (cf. Gazetteer - Saudi Arabia, Washington, 1978, p. 290B). The two remaining basalt stones originate "within the perimeter of the airfield at Azraq in Jordan" (cf. ClNSI, p. 14); this city is located about 83 klm east-souheast of Amman and its coordinates are "31°50'N - 36° 49'E" (cf. Gazetteer - Jordan, Washington, 1971, p. 67A).

2 - The five and 1/5 pages of ClNSI are taken up here as another example showing how pitifully Saf is actually being mistreated not only by unsound or erroneous or unproved statements but also by the avowed myopia of authors who take their own nearsightedness as the supreme and final yardstick of reading. The following notes which could easily have been expanded, were limited to the strictest minimum. How, then, could any reader expect any extensive publication be taken up in the same manner without the writer being forced to devote to it a very large amount of time which can be devoted much more fruitfully to an original research?

3 - The photographs of the stones are clear with the exception of that of the second stone from Azraq (cf. pl. 2b) which is very gray. Furthermore, the facsimiles of the inscriptions, although close enough to the originals, do not in general reproduce faithfully the details of the letters; here, follow four examples.

a - In ClNSI 2 (cf. the facsimile on p. 15a, and photograph on pl. 1a), the drawing of l of slm has incorporated an almost vertical narrow crack on the stone located below the letter itself; thus making the letter almost twice as long as it actually is.

b - The drawing of y of ʾbyn of the same text misrepresents the original: the tracing of the circular loop is too wide, and the

straight shaft became a slight curve.

c - The tracing of n of the same ʾbyn is drawn aslant to the left in the horizontal axis of the lower half of the circle of y. The letter actually is in the axis of the letter height which crosses the preceding y immediately below its upper appendix.

d - In one case, the drawing so mutilates the letter that its value is changed. In ClNSI 4 (cf. facsimile on p. 18a, and photograph on pl. 2a), k of mskn (p. 14) is clear on the photograph, but its diacritical element, a long line aslant to the right, almost parallel to k of ʿky of ClNSI 3, and appended to the center of the left section of the triangular body of the letter, is missing on the facsimile; thus, the letter of the facsimile is "b."

4 - The facsimiles of ClNSI 1 and 2 are printed on p. 15b (bottom) and 15a (top), respectively, instead of on p. 15a and 15b. The two photographs are also reproduced on pl. 1 according to the same disposition, viz. ClNSI 1 at the bottom and ClNSI 2 at the top. The captions of the photographs, however, were given according to the normal location which the photographs should have but have not; consequently, the two captions must be interchanged, viz. "Ai" printed below the upper photograph must be corrected to "Aii;" and "Aii" below the lower photograph must be corrected to "Ai."

5 - Although the seven new texts do not present any special difficulty, many statements made by the editor must be corrected.

B - ClNSI 1 (pp. 14, 16, facsimile on p. 15b, and photograph on pl. 1b).

1 - Preliminary remark. - The second paragraph of the commentary is marred with two printing errors in the personal names, viz. "MacDonald" (with the correct spelling three lines above) and "Winett," nine and three lines from the bottom, respectively.

2 - Clark's opinion on ʾhl.

a - The editor endorses the opinion suggested by JaMAR, VIII, p. 14, on the twofold meaning of ʾhl as tribe and family, and applies it to the end of the present text wtšwq ʾl ddh wʾl ʾhl, which he translates as follows: "and he was filled with longing for his paternal uncle and for his family "(p. 14).

However, Clark does not justify his choice of the meaning here and in any of the other texts of his paper. The study of the nouns expressiong a family relationship (cf. below, the appendix) suggests that both ʾb and dd were the two most im-

portant persons in the familial hierarchy. The absence of ʾb in the present pericope suggests that dd had become the head of the author's family after the much earlier death of his ʾb whom he might not have well known or known at all.

b - More than half of Clark's commentary is devoted to his study of ʾhl in the Saf material, and the reader is given many new examples of the editor's amazing reasoning method. According to him, the word ʾhl is

> " attested seven times in Safaitic (in C 1871, 2192, 2297, 2318, WH 135, HCH 132 and SIJ 837) and the Thamudic text WR 48a may be added to this list. In C 2192, 2297 and 2318 the meaning could be either "tribe" or "family" but in all other cases "family" seems to be the more appropriate rendering:" (p. 16).

These statements are followed by the editor's study of CIS 1871, WH 135, HaCH 132, Wi 837 and his "Thamudic WR 48a." The following remarks are in order.

(1) Clark's list remains incomplete, viz. he fails to mention WiAm Saf 2 (cf. F.V. Winnett, in AION, 31 [1971], p. 452, facsimile on pl. 6, no. 20). The central pericope of the text is whrṣ ʿl ʾhlh (ʿl instead of the edition "gl;" cf. JaMAR, VII, p. 134, note 82) and he was on the lookout for his family. It does not seem out of place to repeat here that JaSICA (1971) noted that the pericope hrṣ ʿl "is a characteristic of the southern Safaitic group" (p. 44).

(2) CIS 1871 - The pericope involved is wʿyd ʾhlh bḥbs which CIS translates "Et festum celebravit familia eius prope Ḥabis "(p. 247).

JaMAR, VIII, states that the present text "favors translating ʾhl as 'family, relatives' because it would be very difficult to prove that the engraver's tribe would have gathered at Ḥabs for the festivities" (p. 15). Therefore, Clark is not the author of his interpretation of ʾhl as family in the present text.

(3) CIS 2192, 2297 and 2318 - Clark fails to note that the three texts have in common an almost identical pericope (cf. JaSN, p. 94, note 160), viz.:

snt wsq ʾhl khl mḥmn in CIS 2192, and
snt wsq ʾhl khl in CIS 2297 and 2318.

In each case, CIS translates ʾhl as "tribus" (pp. 287, 300 and 303, respectively). The three texts come from the same general area, viz. around Zelaf (cf. CIS, p. 199); and this information suggests that they refer to the same event. JaSN translates the pericope as follows: "the year the people of Kahil have gathered

[here] because of the ticks" (p. 94, note 160). The interpretation of ʾhl as family is disproved by the following consideration. None of the authors of the three texts belongs to ʾhl khl, and due to the close proximity of the last two texts, the author of CIS 2318, qdm, is most probably the son of the author of CIS 2297, ʾnʿm. It does not seem possible that those three persons would have paid so much attention to an event affecting a single "family" to which they were unrelated so as to characterize a year by that very limited event.

(4) HaCH 132: the last pericope read on the basis of the facsimile (cf. HaCH, p. 40, lower left corner, and p. 41) is difficult to understand. ClNSI reads the three sections as follows:

> " *wndm 'l bn 'ḫb msby ḥwlt fnwjy fmyt zwy bnf 'hlh*, "he grieved for the son of his brother, a prisoner of HWLT; and he (the prisoner?) was castrated and he died, being carried away into exile (from) his family".[5] " (p. 16).

The first section of the pericope was already read correctly by E. Littmann (cf. HaCH, p. 41) but Clark does not translate the conjunction w; and the second section, fnwgy he was emasculated, comes from JaSN, p. 56, note 57. Furthermore, Clark fails to explain or justify his question mark after his "prisoner." The only other possible subject of nwgy would be the author of the text. But, how could he be the subject of the verb since he himself says that myt he died? JaMAR, XIII, already pointed out that, according to ClAS, a "man [was] informing the passing-by travelers that he is dead in nos. 213 and 278" (p. 18).

Clark's commentary (p. 20, note 5) on his "zwy bnf" contains two examples justifying once more JaMAR's statements according to which Clark's "use of Ar dictionaries is amazing" (p. 17). He writes this:

> " *zwy* — see the Arabic ذوى "to carry away";
> *nf* — see Arabic نفس, "to be exiled, driven away". " (p. 20, note 5).

(a) "ذوّى" - The second form of the verb is not listed by FrLAL, II, p. 100B, LaAEL, p. 989B, SaAAL, p, p. 254A, A. Cherbonneau, Dictionnaire arabe-français (langue écrite), Beirut, 1973, p. 311B, etc. The first form of dawá (i) means "(branch, twig, herb, leguminous plant) to wither, dry up" (cf. LaAEL, p. 989B). But, if his "ذوّى" is a misprint instead of زوى, which is the correct parallel, not "ذوى," the second form of Ar zawá (i) means "to invest, deck or adorn" (cf. LaAEL, p. 1273B). The first form of zawá (i), which is excluded by the editor's tašdîd, means "to put aside, away, apart; to remove from its place" (cf. LaAEL, p. 1273A). Finally, Clark's translation "being carried away" demands a passive mqtl form; thus, "mzwy" should have been engraved

instead of "zwy."

(b) How can "nf" be related to Ar " نفس "? Furthermore, the meaning given by Clark to Ar " نفس " is nowhere to be found in the dictionaries listed above under his "zwy." His " نفس " has to be a misprint instead of نَفَى; cf. Ar nafá (i) "to drive away, expel, banish" (cf. LaAEL, p. 3036C). Not only Clark gives the verb a passive meaning instead of the active, but he also indulges in implicitely repeating the theory that w and y as third radical may disappear in the perfect of the 1st verbal form of a defective verb, and this theory was already rejected as "erroneous" in 1971, JaROx, p.276 (in his ignorance of Saf, W..W. Müller recently repeated the same error; cf. JaMAR, XIV, pp. 149-150). And Clark extends that theory to the nouns: he should have referred his Saf "nf" to Ar nafy "expulsion, banishment, exile" (Cf. SaAAL, p. 1092B). Moreover, in order to give some meaning to his already fallacious interpretation of his "zwy bnf," Clark invents without even mentioning his initiative in his commentary the removal preposition m- (= mn) from which is not to be found in the text, and to better hide his move, he puts his "from" between parentheses instead of between brackets. Such misuse of the parentheses instead of the brackets is also found in his translation of ClNSI 2 (thrice), 3 (once) and 5 (once); yet he correctly uses the brackets in his reading of Saf WiTh 48a, ClNSI 4, 6 and 7. This is another example of Clark's confusion. Indeed, his interpretation of his "zwy bnf" forced him to invent that removal preposition because without it the third section of the pericope would have to be given an absurd translation since ʾhlh would have to be either the determinative of "bnf" or the subject of the verb "zwy;" thus, respectively, "into the exile of his family" or "[he whom] his family had carried away into exile" (for other examples of impossible translations, cf. JaSN, pp. 19-21, and of "absurd translations," cf. my review of WiHaI in JaIFSC, pp. 505-508). Finally, Clark's translation includes an obvious redandancy: what is the justification of adding "being carried away into exile" since the text already stated that that person was msby ḥwlt prisoner of Ḥawâlat? How could he be a prisoner without having been taken away by his raptors?

(c) Clark's erroneous etymologies of his "zwy" and "nf" and his interpretation of the whole pericope implying an inexistent preposition and marred with unjustified difficulties clearly indicate that the copy cannot be without mistraced letters, and it is the difficulty of giving a reasonable meaning to the existent pericope that prompted JaSN (p. 56, note 57) to find a translatable text while disturbing the copy as least as possible

... and this Clark calls " futile speculation." (p. 20, note 5). Such a judgment is another indication that the author does not really know the exact meaning of the words he uses, as already pointed out by JaMAR, XIII, e.g., p. 12. Summarily, I do not find in ClNSI's study one valid reason for altering JaSN's interpretation of the text which reads as follows: "fnwgy fmyt ġwy kf ʾhlh And he [the ex-prisoner] has been emasculated and has died, erring, at the border of his own people" (p. 56, note 57).

(5) WH 135 - The pericope reads fhlt qbl lʾlh slm. JaMAR, VIII, notes that "a similar reasoning [to that on CIS 1871; cf. above] may safely be applied to WH 135 where the goddess Lat is requested to grant welcome to ʾhl slm" (p. 15).

Here again, Clark's opinion of ʾhl family in the present text is not his. Furthermore, the meaning of qbl(l) was determined by JaSICA (1971) as welcome (p.66, on JaS 71a). Following into Winnett's footsteps (cf. WiHAI, p. 643), Clark (cf. also ClAS, p. 474) translates qbl as "acceptance," as if the two words were synonymous. - Cf. below, the note on qbl(l).

(6) Wi 837 - Clark reads the pericope as follows: "wḫrṣ ʾhl(h), "and he was on the lookout for his family"." (p. 16), instead of ʾhly ʾAhlay in JaSN (p. 56, note 57). In his note 6 (p. 20), Clark reproduces a section of the second half of JaSN's commentary on the possible correction of ʾhly (which is clear on the facsimile) to "ʾhl(h)," but he fails to inform his reader that JaSN maintains ʾhly as a personal name. Indeed, there is no reason at all to correct ʾhly because the name ʾhly belongs to a well-attested nominal derivation and does not make any difficulty in the text. Yet, Clark amends it to "ʾhl(h)" without any justification of any kind although he himself had remarked in the immediately preceding note 5 that " any alteration of the reading provided by the published facsimile is futile speculation. " This statement is quite true in his case. His correction is a mere speculation because it remains without any material proof; it is also futile because the text makes good sense without it. Therefore, unless a good photograph is released, there is no reason at all to correct the name ʾhly. It is sad, unfortunately, that a good photograph would not convince him because he could also claim that the engraver made a mistake.

(7) The last remaining text is Saf WiTh 48a (cf. WiReAR, p. 86, facsimile and photograph on pp. 209 and 219, respectively). Clark's study of the text is yet another good example of the confusing argu-

menting method of an author who does not understand what he writes.

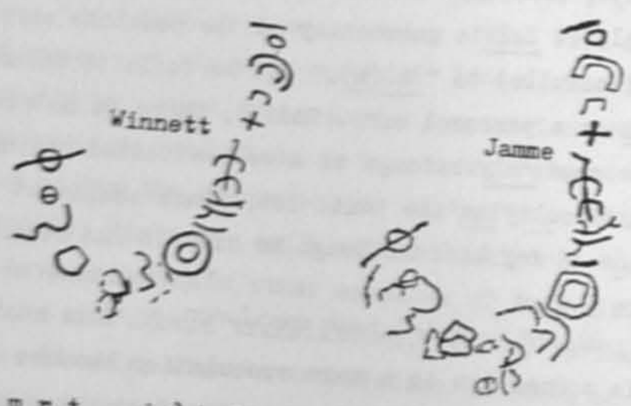
(a) As seen above, Clark states that "the Thamudic text WR 48a may be added to" the "seven .. Safaitic" inscriptions (cf. above, p. 86, for his text). But, surprisingly enough, 14 lines below, he declares that the text

> "is typical of southern Safaitic/northern Thamudic texts, classified simply as *Safaitic* by this writer."

The first part of this statement is a mere repeat of Winnett's remark in the commentary on Saf WiTh 18 (p. 79), and the second is also the repeat of my statement : "all Tabuki texts and one Hijazi inscription are Safaitic" (cf. JaRWiReAR, p. 524A). Thus, Clark apparently is unable to understand the difference between the two sigla "Thamudic text WR 48a" and "Saf WiTh 48a." The first expression, his own, specifies that the actual language of the text is Tham, but the second says that the text is Saf although it was published as Tham by the editor.

(b) The discussion of Clark's note 7 (p. 20) is best preceded by Winnett's facsimile and my own tracing from the photograph, and also by the three readings of the text known so far.

Saf WiTh 48a



Winnett: " l ʿ m r t d ʾ h l ġ f (f) (w š) w q" (cf. WiReAR, p. 86);
Clark: " l ʿ m r t d ʾ h l ḏ[f f w š] w q
Jamme: " l ʿ m r t d ʾ h l ḏ r b b w b ʿ q
"By ʿAmrat, him of ʾAhlaḏ. He has halted and cried [here]"
(cf. JaRWiReAR, p. 525A).

Clark's translation is as follows: "By ʿMRT of the tribe Ḏ[PP and he was filled with longing]."

[1] The most obvious remark to be made is that Clark contradicts himself once more here. He states

that "in all other cases [except CIB 2192, 2297 and 2298] 'family' seems to be the more appropriate rendering" (p. 16, lines 4-5; cf. above, p. 86 for his complete text). Thus, the present text is one of "all other cases" yet, he translates ʾhl as "tribe."

[2] Clark notes that " Winnett's facsimile of this text does not accord with the published photograph in respect of the letter forms and their relative positions on the stone. " (p.20, note 7).

But the letter forms are also distorted in Clark's facsimiles, as already noted above (pp. 84-85). As to the location of the letters on the stones, Clark is no more accurate than Winnett; e.g., Clark completely missed the mark in his facsimile of the lower left edge of ClNSI 2 where the initial l should parallel the left edge of the stone, and the bottom of š, which is composed of three short strokes (instead of two, as in his facsimile) should erase the bottom edge of the stone. Secondly, if his statement is correct, how could he possibly add three lines below that

> "one can do no more than follow Winnett's reading."

?

How can anyone "follow Winnett's reading" if he is convinced that Winnet's facsimile is inaccurate as far as "the letter forms" are concerned?

[3] Clark's first statement reproduced above on "Winnett's facsimile" is immediately followed by this:

> "The letter must be read as ġ, not ḏ."

Which letter he is talking about? It would have been so easy to indicate that he was speaking of the 10th letter instead of the vague expression "the letter." Furthermore, beside his reference to JaRWiReAR, he should have mentioned that the reading of that letter as ġ is explained in JaSICA, pp. 48-49 and the small genealogical tree on p. 611, ġ. Instead, he refers to his thesis, ClAS (p. 74), where he simply endorses without any justification whatsoever the third interpretation listed by him.

[4] Clark then states that "What follows this letter is unclear in both the facsimile and the photograph." And this is his justification of his two immediately following statements which read as follows:

- "one can do no more ... reading" (cf. above, the center of this page) and
- my reading of the text" cannot be justified from either photograph or facsimile. "

Clark's reasoning is nothing but amazing. On the one hand, if he admits that the end of the text is "unclear in ... the photograph," this means that he is unable to read the photograph. But, then, how can he publish the two following contra-

dictory statements, viz. that Winnett's facsimile is inaccurate and that

"one can do no more than follow Winnett's reading."

On the other hand, if he admits that "Winnett's facsimile ... does not accord with the published photograph," he has no foot to stand on to reject another reading based on that photograph. Yet, the reader can see that Winnett himself corrects no less than three letters and eliminates another one which he draws in stippling. The most Clark could have done is to write this: "I can do no more than follow Winnett's reading" and everybody would have understood his avowal of incompetence. Clark's assumption that the last section of the text is "unclear in ... the photograph" comes from the fact that the space occupied by the last third of the text contains many curve and half-shell features some of which are incorporated in both Winnett's facsimile and Clark's reading. Also, the penult letter is much too small compared with the w of the text to be read "w" (cf. both Winnett and Clark), and the crossing bar recorded by Winnett belongs to a long line protruding on both sides of the letter which is c.

c - The preceding remarks on the Saf texts with ʾhl may be summarized as follows:

	ClNSI	Jamme
CIS 1871	"family" as in ...	CIS and Jamme: "family"
CIS 2192, 2297, 2318	"either 'tribe' or 'family'	CIS and Jamme: "tribe"
ClNSI 1	"family"	"family"
HaCH 132	"family"	"tribe"
WH 135	"family"	"tribe"
Wi 837	"family"	(the word does not exist)
Saf WiTh 48a	"tribe"	(the word does not exist)
WiAM Saf 2	(not referred to).	"family"

d - Note on qbl(1) (cf. above, p. 89 on WH 135). qbll is the last word of JaS 108 (cf. JaSICA, p. 78, and facsimile on p. 623). The name mentioned in the second pericope of the text is ʾtrṣ ʾAtraṣ. On the misreading of t for h, ClAS writes this:

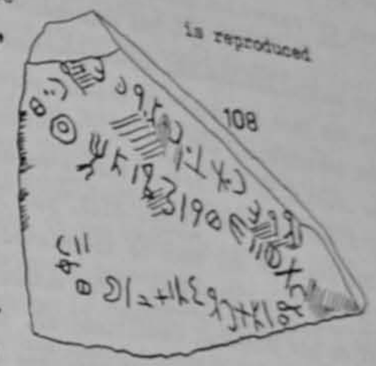
" This mistake may have been made by A. Jamme in JaS 108, where we should read *whrṣ ʿl ʾhrṣ*, "and he was on the lookout for 'HRṢ". Jamme has read the name here as 'TRṢ" (p. 82, note 38).

So reads the Ann Arbor 1983 printing. In the original text of the thesis, however, the above-reproduced remark is immediately followed by this:

" If photographs of Jamme's texts were available this matter could be properly decided."

(1) Where in the Ann Arbor 1983 printing does Clark notify his reader that some corrections were made to his 1979 text before the latter was sent to Ann Arbor and why does he not give the complete list of those amended passages? As a matter of fact, the reader will never know the reason behind each deletion or correction.

(2) Here is reproduced my tracing made from my Kodachrome (cf. JaSICA, p. 623): both the sizes and the forms of the two h's of hzn and hrṣ and the two t's of ʾtrṣ and lt are clear. However, if by any chance, Clark wishes to labor the slight difference between the two t's, the reader wonders why he should fuss so much about it since his basic reading principle is as follows:



" all of the variant forms listed by him can be explained as accidents of engraving. " (p. 83, note 51; same text in the thesis).

With such a principle, Clark will of course be able to read everything he wants.

(3) What is the reason behind his fussing about photographs? Nowhere does he say that his own photographs whose "the colour slide ... was generously provided by the Agfa-Gevaert Company" (p. iii) were available to ALL scholars for the asking? They were, and still are, I am sure, available to his friends Winnett, Macdonald, etc., as the original photographs by Winnett, Harding, Macdonald, Knauf, etc., are available to the friends of each of them, not to ALL scholars. My stand on that matter has always been that copies of my photographs are available to all scholars but not on a one-way street basis.

C - ClNSI 2 (pp. 16-17, facsimile on p. 15a, and photograph on pl. 1a).

1 - Although duly registered in the facsimile, the set of seven strokes located in the eye of the spiral made by the text is not mentioned in the commentary.

2 - The verb ṣwy is translated by Clark as "it(the herbage) has withered" (p. 16), and the commentary (pp. 16-17) does not contain a single word of justification as to the subject of the verb choosen by the editor. In the four texts known so far with the verb ṣwy, viz. JaS 61 (cf. JaSICA, p. 63), CIS

1686, Wi 90 (for both texts, cf. JaSN, pp. 39–40, note 24) and Irbid 1812b (cf. JaMAR, XIII, p. 144), I have consistently translated the verb as having the author of the text as subject. The reason is obvious, but Clark does not seem to be aware of it, viz. when a verb immediately following the pedigree has a meaning applicable to humans and especially when it is introduced by *w* and, the subject of the verb is normally the author of the text itself. The expression *wgwy wdt²* of the present text makes the preceding remark even more compelling because the verb *dt²* cannot but refer to the same subject, viz. the author of the text.

3 – The expression *flt wdšr slm d²l yškr* is translated by Clark as "So Lat and DŠR (grant) security to him of the tribe of YŠKR" (p.16). Since the verb "(grant)" is between parentheses, it is supposed to be restored in the inscription, and *slm* is a substantive. This interpretation is disproved by the absence of the preposition *l* before *d²l*. Here, *slm* is a verb (as, e.g., in CIAS 316 and 1029; cf. JaMAR, XIII, pp. 52–53 and 92, respectively) and in the dual. The pericope may thus be translated as follows: And, O Lat and Dūšarâ, save him of the tribe of Yaškur. The author's prayer is quite natural after his statement on his physical condition; cf. the verb *gwy* he has withered.

4 – The last pericope of the text is read by the editor as follows:

" *wl't lt mn yḥblh*

and may Lat torment whoever may damage it (the inscription? the tribe?)". " (p. 16).

a – In his reading of "*l'ct*," the editor disregards the letter *n* between *'* and *t*, which is even clearer than the second *n* of *bnsnt*. Thus, the feminine verbal form *l'nt*, cf. *l'n*, e.g., in CIS 3952 (for the passive in CIS 2286, cf. A. Jamme in JAOS, 91 [1971], p. 138A, note 5).

b – Although his commentary refers *yḥbl* only to LP 684 (p. 17), where the verb *ḥbl* is translated as "to destroy" (cf. LiSI, pp.175 and 316B; cf. Ar *ḥabala* "to corrupt, render unsound, vitiate, or disorder one's reason or intellect, or one's limb or member" in LaAEL, p. 699B), Clark chose Winnett's interpretation of "to damage" in WH 181 (cf. WiHaI, pp. 74 and 634) as if the two verbs were synonymous. Furthermore, there can hardly be any doubt that *h* of *yḥblh* refers to the inscription, not to the tribe, because, although the direct complement of *ḥbl* may occasionally be an individual (e.g., Wi 819), it is normally the text itself; e.g., *ld ḥbl ḥtl* of CIS 1087;
lmn (y)ḥbl ḥsfr of CIS 2276; and
dḥbl ḥḫt(ṯ) of Wi 841.

c – In order to prove that
" The use of the relative pronoun *mn* is not common and it would be more usual to find *ḏ* or *ld* employed " in the preceding pericope, Clark refers to two irrelevant expressions since they involve the verb *'n*, not *bnt*. In fact, *mn* is used thrice so far, viz. in CINSI 2, CIS 1186 and 2276 (see above), and *ḏ* in eight texts, viz. the two texts listed above, CIS 3952, RAMAT 5, LP 142/6, 360, 684 (for J 13206f, cf. JaMAR, XIII, p. 146–147) and WH 181. Thus, Clark's remark is correct, but he should have given appropriate examples.

d – The last pericope of CINSI 2 may be read as follows: *wl'nt lt mn yḥblh* And that Lat may curse anyone who would destroy it [= the inscription].

5 – Wi 819: the last 2/3 of the text is correctly read by WISIJ (p. 112) as follows: *wwǵm 'l ²ḫth sdt mḥt fḥblh*, and his interpretation, which is endorsed by R.M Voigt (cf. ADAJ, 24 [1980], p. 87), is "And he grieved for his sister, Sudat, who had passed away. And it has mentally deranged him." Such an interpretation is unacceptable as obviously impossible. Who could an author compose and engrave an inscription when he is "mentally deranged"? I suggest translating the text as follows: And he has mourned over his sister Sudat [who] went down a well and it destroyed her. — *mḥt*, cf. Ar *maha* (i) "descendit in puteum implendae urnae gratia, nempe cum pauca est aqua" (cf. PrIAL, IV, p. 222B). — Two more remarks are needed. The seventh letter is *ś*, not a doubtful "(*š*)" (cf. JaSVW, p. 166) as in the edition; cf. the genealogical tree of the letter in JaSICA, p. 612. — M. Rodinson deals with the above-quoted pericope of Wi 819 and states that, instead of the editor's translation, "Je suggérerais de comprendre plutôt: 'il a été irrité (*muḥita*) et il l'a blessée (*ḥabala-hā*)" (cf. Arabica, 6 [1959], p. 219).
This is another example of the rash temerity of an author who, under the fallacious pretext of knowing Ar (cf. already above, Serjeant's example on pp.65–69), believes that he is competent in Saf also. Even at first sight, his translation is erroneous. The word *mḥt* has to be an apposition to *sdt*, either a noun or a verb; *ḥbl* means to destroy, not "to wound;" finally, it would be absurd for the author of the text to say that he mourned over his [DEAD] sister and, AFTER THAT, to confess that he "wounded her."

D – CINSI 3 (p. 17, facsimile on p. 18a, and photograph on pl. 2a).

1 – The verb *gyr*. – The pericope *fgyr bnngm* is translated by the editor " and went back to the encampment at HNJM ," and his commentary adds

" or we may translate "at the appointed time". "

Clark notes that ŝyr "also occurs in WH 77." The verb is so well attested that it is difficult to figure out why a single reference and "WH 77" in particular were selected as if it was the only one. Furthermore, the author translates it as "to go back to the encampment" although he had interpreted it as "to be on a journey" in his thesis (cf. ClAS, p. 473) where he had referred to "C 62"(cf. ClAS, p. 234); and CIS's commentary on CIS 62 (p. 14B) refers to "arab. ŝayara 'pervenit, venit'; i.e. pervenit ad locum in quo excisa est inscriptio." Clark seems to justify his change of mind by referring to

" Arabic صار, "to return, to go back to an encampment". "

Clark does not specify whether his "ŝâra" belongs to the root ŝwr or ŝyr; nor does he explain the reason of "an encampment" becoming "the encampment" in his translation as if there could not have been several encampments or camping sites at Hanaǧm. Since Clark never gives the reference to the dictionary where he is supposed to have picked up his translation, the reader cannot pinpoint the place where his translation of Ar ŝâra comes from. It may well be J.G. Hava (cf. Arabic-English Dictionary, Beirut, 1970) who gives the second meaning of the verb as "To go back to the encampment" (p. 411A) = J.B. Belot's (cf. Arabe-français, Beirut, 1971) second meaning "Rentrer au gîte" (p. 427A) = SaAAL's third meaning "Returned, returned home" (p. 467B). If such is the case, it is important to point out that the selected meaning is never the first meaning of the verb and also that there is no justification in changing "the encampment" into "an encampment." The idea of "an encampment" could have come from FrLAL (II, p. 536B) where the third meaning is given as "Rediit at locum habitationis pabulum quaerens." But, here again, the meaning is not the first one. The idea of "encampment" is secondary to the root ŝyr: it is not mentioned by either LaAEL (pp. 1753C-1754B) or A. Cherbonneau (cf. Dictionnaire ..., p. 548); and LaAEL lists maŝîr as "a place of destination; a place where people alight and abide; or a place of herbage, or pasture, and of water" (p. 1755A). The verb ŝyr simply means to migrate, and the place where the author migrated is the place where he wrote the text (cf. CIS, above), viz. Hanaǧm in the present text. The pericope may be translated and he migrated [here] at Hanaǧm.

2 - The two petroglyths. - The introduction to the publication of ClNSI 3 mentions two separate figures which are described by Clark as

" There is a lively drawing of a horseman and a humped, long-horned animal on the same stone." (p. 17).

The second animal remains unidentified and the relationship between the two figures is not commented upon. The horseman on the right, a hunter, whose trunk faces the onlooker, brandishes above his head a very long spear held in his left hand, but the drawing of his mount is rather clumpsy with an uglily wide neck and an unusually long and thick tail. The animal on the left, the beast hunted by the horseman, is an Arabian oryx (cf. D.L. Harrison, The Mammals of Arabia, London, II [1968], pp. 344-345). The triangular-shaped protuberance immediately next to the neck is not a hump, but the slightly exaggerated design of the withers. The left half of the hunter's spear overhangs the whole figure of the oryx. According to Clark (p. 17),

"These drawings were executed before text a, but are perhaps contemporary with text b."

Of course, nobody knows the exact meaning Clark gives to "contemporary." That ClNSI 3, his text "a," was engraved a f t e r the tracing of the two figures is proven by the disposition of the letters aside the legs of the animals. That the engraving of ClNSI 4, his text "b," also is p o s t e r i o r to that of ClNSI 3 is proven by the letters of the first hemming the curves of the second and ending just short of the last loop of the same second text. Thus, the chronological sequence of the three items is certain and Clark's "contemporary with text b" is erroneous because the figures even precede ClNSI 3. Clark's connection of ClNSI 4 with the two figures is apparently based on his suggestion of "ʿr" at the end of the text. It is difficult to understand why Clark does not refer to the second animal on the left of the horse. Is it because he does not identify it? At any rate, since none of the two figures represents an "ass," and ClNSI 4 was engraved even after ClNSI 3, there is no reason whatsoever to even suggest any relation between any of the two figures and either ClNSI 3 or 4.

E - ClNSI 4 (pp. 17, 19, facsimile on p. 18a, and photograph on pl.2a).

1 - The editor reads the beginning as follows: " / [ḥḥ]b

"By [HB]B."

The first section of the commentary reads as follows:

" The first name may be the well-known HBB.[13] "

Note 13 is as follows:

"[13] See V. A. Clark, Thesis, op. cit., text no. 81, for a discussion of the personal name HBB. " (p. 21).

The name "ḥbb" cannot be read on the photograph (cf. below), so that the name "ḥbb" is well-known does not by itself prove that it may be found here.

a - CLAS's thesis is presented as follows:

> "In fact, in many instances in which the word Abb occurs and has been read as a personal name it is in fact a common noun meaning "friend, loved one". Both Littmann (LP 328, 359) and Oxtoby (ISB 67) recognize this" (p. 188).

(1) The initial statement is worthless as both vague and unsubstantiated by even as much as the hint of a proof, and all details on those "many instances" remain unknown. Finally, the redundancy of the second "in fact" is obvious.

(2) E. Littmann did not "recognize this," as declared by Clark. In LP 328, LiSI (p. 82) translates wwgd sfr ḥbb as "And he found the inscription of Habib (or: of a friend)." It seems quite obvious that Littmann did not recognize the exclusivity of Clark's thesis since he still gives his preference to ḥbb as a personal name. Furthermore, LiSI's commentary is worthless; it reads as follows:

[1] " *Habīb*, a very common name, means "friend". [2] It is scarcely probable that the author found an inscription of his own. Occasionally we find the statement that a man found the inscription of a relative or of a fellow of his without the addition of a proper name. " (p. 83, commentary on LP 328).

Ad [1] - The statement is irrelevant. It is not because ḥbb may be a substantive that such is the case in the present text.

Ad [2] - Littmann's statement is "not scarcely probable" but it is indeed most obviously impossible. That ḥbb of sfr ḥbb cannot possibly be the same as the author of the text is evidenced by the last pericope fnʿ and he [= the author of the text] was afflicted because this condition is always connected with a deceased person and prompted by the finding of the latter's inscription.

Ad [3] - Littmann misrepresents the Saf material in order to foster his preconcieved theory, viz. he hides from his reader the fact that the word designating "a relative or . a fellow of his without the addition of a proper name" a l w a y s is determined by the personal pronoun -h his. Here follow the examples from the very collection of texts Littmann was working on when he wrote his comment on LP 328 and especially LP 332 which is found one single page only after LP 328, viz.:

LP 332, 382, 416, 562a, 697 and 723: wwgd sfr ʾbH and he found the inscription of His father;

LP 342: wwgd sfr ʾḫyʿH and he found the inscriptions of His companions;
LP 368: wwgd sfr ʿmH and he found the inscription of His grandfather;
LP 371 and 563: wwgd sfr ḫlH and he found the inscription of His paternal uncle;
LP 395: wwgd sfr ʾḫH wʾḫyʿH and he found the inscriptions of His brother and of His companions;
LP 412 = 1038: wwgd ḫṭṭ ʾḫH and he found the writing of His brother;
LP 443: wwgd sfr ʿmH ... wsfr ddH and he found the inscription of His grandfather ... and the inscription of His paternal uncle;
LP 1300: wwgd sfr ddH (cf. my paper in Orientalia, 39 [1970], pp. 415-416): and he found the inscription of His paternal uncle;

The preceding construction is also used when ʾḫr replaces either sfr or ḫṭṭ, viz.

LP 154 and 156 (compare LP 1300): wwgd ʾḫr ddH and he found the trace of His paternal uncle; and
LP 306, 315, 399, 408, 438, 583, 590 and 610 (compare LP 342): wwgd ʾḫr ʾḫyʿH and he found the traces of His companions.

Beside misrepresenting the Saf material, Littmann (and, therefore, any of his followers, including Clark) is inconsistent with his own theory. If his [3] is valid, what excuse does he have to translate either dd as "Dād" instead of "a paternal uncle" in LP 259 which reads as follows: "wwgd [ʾ]ṯ(r) dd wmʿ and he found the inscription of Dād and longed [for him]" (p. 65), or ʿm as "ʿAmm" instead of "a grandfather" in LP 1101, where he reads the complement of wgm as "ʿl ʾḥ(ḥ w)ʿl ḥn wʿl ʿm of his brother and of Ḥunn and of ʿAmm" (p.249)? Why does he not translate the latter pericope as "of his brother and of an affectionate one [cf. Ar ḥanūn] and of a grandfather"? For this text, cf. below. The same remark must be made wherever, e.g., dd, ʿm, etc., is a complement of a verb expressing feelings, e.g., wgm ʿl; e.g., CIS 4443: wwgm ʿl ʾḫh wʿl ddh wʿl ḫlh wʿl ʿm.

(3) - W.G. Oxtoby translates wwgm ʿl ḥbb of Ox 67 as "And he grieved for a friend" (cf. OxSI, p. 50, and facsimile on pl. 5; for my review of this book, cf. JaROx), and his commentary reads as follows (p. 50):

> " The editor of the Corpus consistently renders ḥbb without the article as the name Ḥabīb even when the result is difficult to understand (cf. C 2489 with L 359 and 398); ḥabīb as a noun is recognized by Winnett in W 438."

Indeed, CIS's commentary on CIS 23 (p.68) states that "ḥbb, absque suffixo, pronomine usurpatur." It should be noted here, however, that CIS himself hesitantly

makes an exception to that rule in a parallel expression in CIS 5044, where he translates wgm ʿl bnyt as "Et posuit signum pro filiola(?)" (p. 617). But, the commentary adds on bnyt: "forsan, deficiente pronomine suffixo, tamquam n. pr. interpretandum." As I stated earlier (cf. JaGUR, p. 33), "bnyt is certainly a personal name."

The four texts referred to by Oxtoby are these:

CIS 2489: CIS reads the end of the text as follows: "wngʿ ʿl hbb fhbb whrg mhʾmn ml Et dolore affectus est erga Habîbum. Et Habibus, et expectatus est a fido Maylo" (p. 323);

LP 359 and 398: LiSI reads the last pericope as follows: "wwgm ʿl hbb f hbb And he laid a stone on the tomb of Habib in friendship" (pp. 94 and 104, respectively); and

Wi 456: WiSIJ reads the last pericope as follows: "wwgm ʿ(l) ḥ(b)b fl And he grieved for a friend, FL" (p. 70).

Oxtoby, followed by Clark, bases his whole case on translations which even at first sight are unacceptable and eliminates two of my publications. Unacceptable translations cannot by themselves prove that Oxtoby's view is correct, and it is very regrettable that Oxtoby does not translate the three texts according to his theory because his translations would have been as unacceptable as CIS's and LiSI's.

(4) CIS 2489 - CIS's commentary on the end of CIS 2489 is wholly unacceptable. Firstly, there is no mention at all or explanation on the very strange section "Et Habîbus." Secondly, the interpretation of mhʾmn is farfetched; it reads "ʾmn, cf. arab. ʾamîn, 'fidus; ille cui gestio committitur'. - ml est appositio; ʾmn articulo praeceditur" (p. 323). There is no reason at all for dissecting mhʾmn into three parts, viz. m (the preposition mn) + h- (the article) + ʾmn (a substantive). Instead, mhʾmn is the active particple of the 4th form of the verb ʾmn, viz. confidant. The pericope may be translated and he was on the lookout for Mayl's confidant.

(5) Wi 456 - WiSIJ's interpretation of Wi 456 contains a contradiction, viz. an undetermined noun having a personal name in apposition. My 1967 comment reads as follows:" Were it the case [viz. fl being apposed to hbb], Saf would surely have hbbh 'his friend', before fl" (cf. JaSVW, p. 166, where "(w)skm" and "(W)askum" should be corrected to "(m)skm" and "(M)askum, respectively), and my translation of the pericope which introduces the comment is "And he mourned ov(er) the fr(ie)nd of Fall." Winnett's preceding novelty is also repeated in WH 2147 where WiHaI (p. 331, and facsimile on p.699) translates mrʾ mtl wmrʾt wʿl as "a man [named] MTL and a woman [named] waʿl" in-

stead of the man of Mâtil and the woman of Waʿl.

(6) LP 359 and 398 - I translated the pericope as follows: "And he has mourned upon Habîb and he was loved"(cf. Orientalia, 39 (1970), p. 413) and my commentary clearly states that "the subject of the verb [ḥbb] is the deceased person."

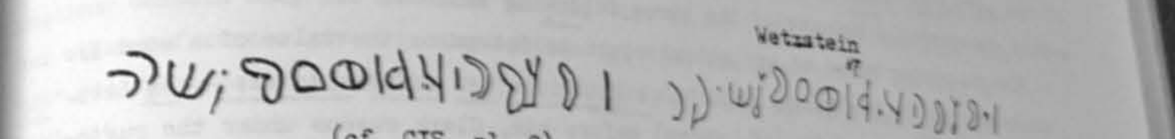
CLAS (pp. 189-190) suggests the same interpretation for LP 359 and 398 without any reference to my paper. He also applies it to both CIS 2489 (cf. above) and 1822. This meaning, however, is incorrect in CIS 1822 (cf. JaSN, p. 102, app. II).

The pericope of Ox 67, wwgm ʿl ḥbb, means and he has mourned over Habîb.

The study of CIS 1142 may be added here. The text is read by CIS (p. 152B) as follows: "lʿmhm bn sʿdl wwgm ʿ[l] ḥbb ʿAmmhumo, filio Saʿadli. Et signum posuit pro Habibo," and the commentary states:"Ita apud W. [= Wetzstein] (praeter ḥbb quod legitur ḥrb)."

Here follow the two copies of the text:

Vogüé 225



(cf. CIS, pl. 9)

Wetzstein

(cf. CIS, pl. 21)

CIS's reading of the second half and the commentary are unacceptable. In fact, only bn sʿdl comes from Wetzstein. But, ʿmhm comes from the combination of the two copies; "w" before wgm does not exist in either of the two copies; wgm is found in both copies; the word "ḥbb" comes from Vogüé instead of Wetzstein's impossible "ḥʿkb;" and, finally, CIS's etymology of ʿmhm dissecting it into two elements, ʿm + hm, cannot be justified as the nominal formation qtl+m is well attested; cf. ʿmh in WH 3593. The text may be read as follows:

lʿmhm bn sʿdl wgm shbb

By ʿAmhum, son of Saʿadil. Ṣaḥbab has mourned [here].

shbb, cf. shb, e.g., in Ox 436.

b - ClAS's thesis is followed by a remark (p. 188) which reads as follows:" Winnett is inconsistent in that he renders the expression wgm ʿl hbb as "and he grieved for Habîb" in SIJ 216 and 217 but as "and he grieved for a friend" in SIJ 456. The only difference between these three texts is the addition of the verb fl, "he was journeying in the desert" to SIJ 456"

The reference to Wi "216" is an error instead of 126. ClAS justifies the remark

of his first sentence by the second. It is strange that Clark gives fl another meaning in ClAS 146, viz. "to escape" (p. 208; cf. also in his "Lexicon," p.474). This change remains unjustified and even unmentioned. — The interpretation of fl as a verb instead of a personal name is disproved by the fact that it immediately (without any conjunction) follows a word which is even at first sight in the status constructus. — The Saf verb fl to escape is also attested in ClAS 700 (cf. JaNAR, XIII, pp. 72-73). — For Wi 456, cf. above, pp. 100-101.

C - After that ClAS applies his theory to the expression wgm ʿl ḫbb. He writes this: " In the case of the expression waǧm ʾl ḫbb it seems likely that we should consider ḫbb as a common noun as the occurrence of this word in this expression is far more common than that of any other personal name in the same context. " (p. 188).

The preceding statement is strictly limited by the author himself to the sole expression wgm ʿl ḫbb, and his presentation is also restricted to the actual count of the word ḫbb to the exclusion of any other consideration. Clark's position is wholly unacceptable because of the three following reasons:

- His reasoning based on any actual count to determine the value of a word is contradicted by the famous principle: quantitas non mutat speciem; — as Littmann, Winnett and Oxtoby (cf. above) before him, Clark sweeps under the rug the basic characteristic of the use of wgm ʿl which contradicts his theory; — and the geographical area with the greatest concentration of texts with ḫbb and the importance of one ḫbb explains the diffusion of the name.

It must be repeated here that the general rule governing an expression such as wgm ʿl (cf. above, pp. 98-99) is so consistent and so well attested that to ignore it cannot be attributed to a simple lapse of memory but, on the contrary, has to be the result of a voluntary act of exclusion. E.g., in the largest collection of Saf texts, CIS, the complement of wgm ʿl a l w a y s is either a name or a noun in the status constructus. Here follow examples involving a substantive, viz.

ʾbH HIS father in 13 texts; e.g., CIS 83, 1252, 1689, etc.;
ʾḫH His brother in 17 texts; e.g., CIS 303, 845, 935, etc.;
ʾmH HIS mother in 8 texts; e.g., CIS 1158, 1159, 1950, etc.;
ʾsyʿH HIS companions in 8 texts; e.g., CIS 905, 913, 1758, etc.;
ddH His paternal uncle in 7 texts; e.g., CIS 1573, 1684, 2381, etc.; and
ḫlh HIS maternal uncle in 7 texts; e.g., CIS 1275, 1813+1812, 1950, etc.

d - The geographical distribution of the texts mentioning the name ḫbb suggests that the name was especially borne in the country of Zelaf from which most of the CIS inscriptions originate.

(1) At the southern fringe of the Saf territory, viz. in the country of al-ʿArʿar in Saudi Arabia, the name appeared once only, in JaS 162b: lḫbb bn ... By Ḥabīb, son of ... (cf. JaSICA, pp. 93-94 and tracing on p. 634).

(2) No ḫbb is recorded in the Saf texts from northwestern Saudi Arabia edited by WiReAR or in the collection published by Y.M. Abdallah (cf. an-Nuqūš aṣ-ṣafawīyat min maǧmūʿat ǧamīʿati al-Riyāḍ ʿām 1966 [Thesis], Beirut, 1970). The editor reads the initial and last names of ANES 120 as "ḫbb" and "dl" (p. 234, and facsimile on p. 231; cf. also pp. 245 and 247, respectively). The text was independently published in JaSICA (pp. 130-131, and tracing on p. 139) as JaS 41; the two names are, respectively, ḫrb and ḏln.

(3) As we move north into Jordan, the name ḫbb starts to appear more often.

The collection published by HaCH does not contain any mention of ḫbb. The Wi inscriptions contain only two mentions of ḫbb, viz. Wi 456 (cf. above) and 955. The four other references given in the edition (p. 155) are inaccurate: instead of "ḫbb" read ḫbr Ḥabir in Wi 126, 127 (cf. JaSVW, p. 164) and 665, and ḫrb Ḥarb in Wi 441.

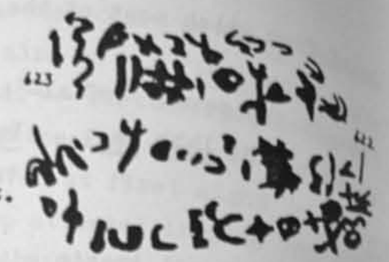
(4) In the large collection published by WiHaI, HaAIC reads "ḫbb" in eight texts and lists only WH 545 and 1799 (p. 172). Nobody will ever know which is his eighth text as WiHAI refers to seven only. In that collection, I know so far of four references only, viz. WH 622+623, 1799, 2379 and 2439. The name read "ḫbb" by Winnett in WH 545, 2520 and 2520a is ḫrb Ḥarb, and the "ḫbb" in the edition of WH 2706 is ḫrr Ḥarīr. Furthermore, belonging to the same area as the preceding collection, comes another example of ḫbb in the expression wgm ʿl ḫbb of Ox 67, where the editor erroneously translates it as a noun, viz. "a friend" (cf. above, p. 99).

WiHaI's commentary on WH 2520 (p. 376) states that the isolated name ḫbb of WH 2520 "appears twice on the rock." The second engraving does not receive any siglum and is not listed under the name (p. 567A). I give it the siglum WH 2520a. These two texts have in common the same author.

The pericope of WH 622+623 is read in WiHaI (p. 137, and facsimile on p. 673) as follows:

" whlt slm ḫrb fṣyt dn lḫbn
w'sd
so, O 'Allāt, [grant] security. In war deliverance

give to Ġuḍain and 'Asad."



The phrasaeology of this translation is not Saf at all: his "ḫrb" has to be the complement of slm. The editor also misreads "ḫrb" instead of ḫbb; the form of r is clear in the 21st letter of the text in brkt. Finally, the initial letter of his "ġḍn" is l: Winnet wanted to find the repeat of his "ġḍn," the author's name. But, the name of the author is ġḍnn, not "ġḍn" as in the edition: the second n was added above the space between "ḍn" and "bn." — The pericope may be read as follows:

whlt slm ḫbb fṣyt dšn llḍn wʾsd And, O Lat, save Ḥabîb. And Dâšin called out to Laḍḍân and made trouble.

ṣyt, cf. Ar ṣâta (o) 2nd form, ṣawwata bihi "he called out ,cried out, or shouted to him" (cf. LaAEL, p. 1742B); the verb is isolated in CIS 5275 in JaSVW, p.161. — llḍn, e.g., llḍ in CIS 2789. — ʾsd, CIS 3432 in JaSN, p. 76. The verb is understood as a verb instead of another personal name because the preposition l is not repeated before it.

(5) As we now move into Syria, the mention of ḫbb increases substantially.

(a) LiSI introduces the theory of ḫbb as "a friend" in the expression wgm ʿl ḫbb as he faces many mentions of the name ḫbb. The origin of his theory, in my opinion, is the pericope of LP 620 where ḫbbh is translated "his friend" (p. 160). This translation is accurate because of all the parallels already given from CIS (cf. above, p. 102), and the LP collection offers more examples of the same:

wgm ʿl ʾbH in LP 235, 338, etc;
wgm ʿl ʾšyʿH in LP 366, 369 and 1157;
wgm ʿl ʾbH wʿl ʾmH wʿl ʾḫH wʿl ḫlH in LP 405/3-6;
wgm ʿl ʾḫtH in LP 540 and 567;
wgm ʿl ḫlH wʿl ddH wʿl ʾmH in LP 605; and
wgm ʿl ʾḫ(h) in LP 1101.

LiSI applies his theory on ḫll to six texts. — LP 359, 398, 444 and 694: ḫbb is translated as "Ḥabîb" and the commentary alone mentions that it "might" (p. 94) or "may" (pp. 119 and 177) be "a common noun 'a friend'" (p. 94). For LP 359 and 398, cf. already above, p. 100. — LP 328, however, is the only case where the two interpretations are given in the translation itself (cf. above, p. 98).

LiSI also applies his theory to LP 255: wwgm ʿl ḫhll which he translates as follows: "And he laid a stone on the tomb of the friend" (p. 62). The phrasing of his sentence does not make sense, and an expression, such as "the friend" is not Saf either; Saf would have ḫllh "his friend." His commentary, however, mentions that "the last word may also a proper name ha-Khallîl." This undoubtedly is the only translation possible. Note also that the order of the contents of translation /commentary is the reverse of what it is in the first four above-mentioned texts. LiSI's vocabulary lists the name ḫbb in LP 137, 274, 303, 304, 328, 393, 508 and possibly 1125. "ḫbb" of LP 274 must be read ḫrb on the basis of CIS 3455; for LP 303 and 304, cf. below, the genealogical tree in CIS; "ḫbb" of LP 393 and 508 is to be read ḫbr (cf. JaSN, p. 112, note 13) and "ḫrb" in the edition of LP 1125 must be corrected to ḫbb.

If we now turn our attention to the pericope wgm ʿl selected by Clark himself, we see that the facts flatly contradict his theory, viz.

wgm ʿl ḫbb is found thrice, viz. in LP 359, 398 and 444;

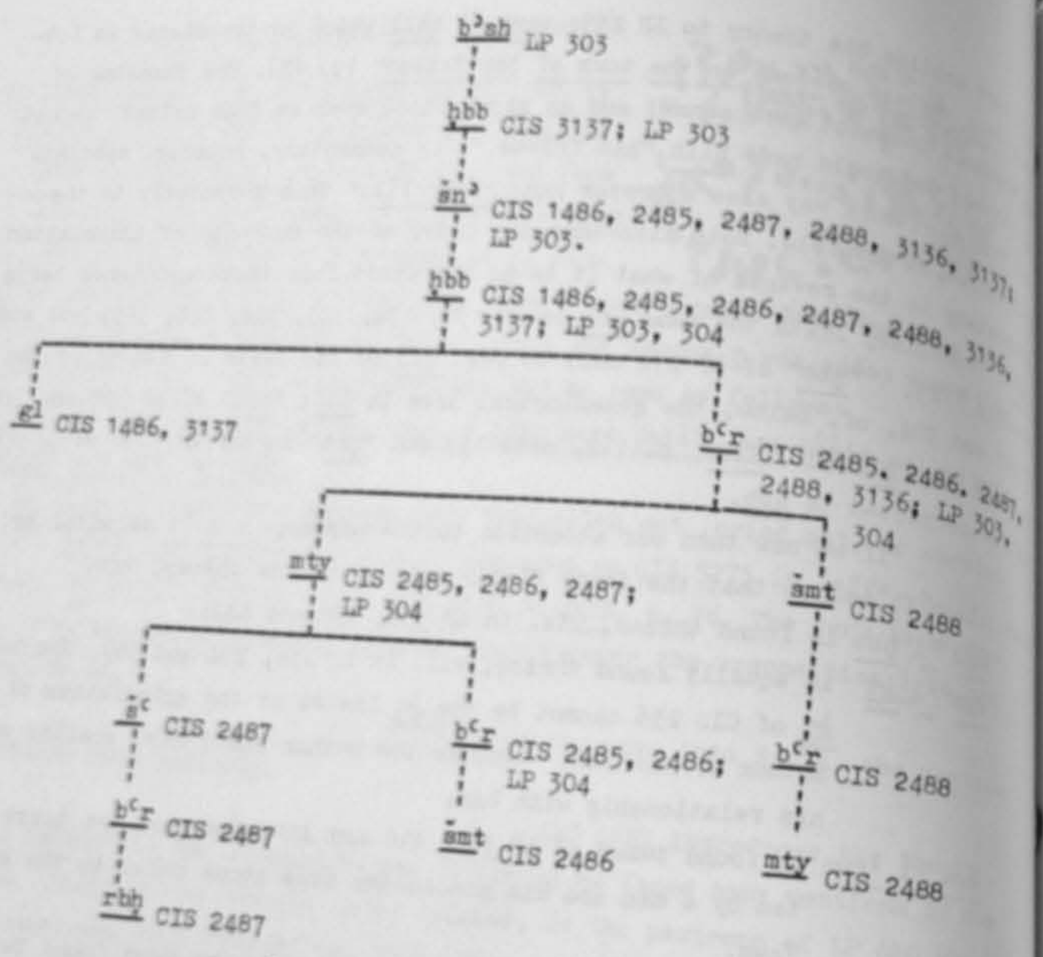
wgm ʿl ġt is equally found thrice, viz. in LP 236, 254 and 569. The name ġt of CIS 236 cannot be the ġt listed as the grandfather of the author of the text because the author would have spelled out his relationship with him.

wgm ʿl ʾqwm is found twice in LP 356, 410 and 411. The last two texts written by a man and his son on the same stone refer to the same ʾqwm.

The four following names, mlk, mḥlm, glḥn and mqm, are each found twice also in the same expression, viz. in LP 461 and 653, 156 and 751, 653 and 658, and the very same LP 308.

(b) The large collection of the CIS texts the greatest part of which originates from the general area of Zelaf shows a great increase in the mention of the name ḫbb.

The name ḫbb is found as an isolated name (CIS 509) and as a part of several pedigrees. The most important of those pedigrees is that of ḫbb of CIS 1486 which is presented on the following page. Beside the pedigree of the next page, ḫbb of CIS 2093, 2263, 3321, 4016, 4561 and 4599 is a different person in each case because the father is different; and each ḫbb of CIS 2207 and 3165 is also different from all the preceding ones. Furthermore each ḫbb of CIS 1047, 1494, 1822, 2201 and 2292 belongs to a phrase and cannot be identified with any of the preceding names.



Geneaological tree of hbb

(cf. CIS, p. 145B on CIS 1081) with his homonymous grandfather and four b'r's, two mty's and two šmt's among his descendants, involving CIS 1486, 2485, 2486, 2487, 2488, 3136, 3137, LP 303 and 304. As JaSN (p. 5) demonstrated how unsafe it is to identify names and the basis of a two-name pedigree "unless," as pointed out by JaSICA, "the pedigree is further defined by either the clan name or the origin of the stone," the following pedigrees hbb bn šn' of both CIS 1372 and 2227, and b'r bn hbb of both CIS 1081 and 3478 cannot be incorporated into the preceding genealogical tree.

CIS 1047, cf. below. — CIS 1494: hbb is the complement of slh he armed. — CIS 1822, cf. above, p. 101. — CIS 2201: "hbb" of the edition must be corrected to hrr Harîr (cf. JaMAR, IV, p. 143, note 123). — CIS 2292: the central pericope reads whkr hbb and Habîb was greatly startled (cf. JaSN, p. 90, note 154).

(6) Thus, so far, the name hbb appears 19 (nineteen) times in the texts outside the pericope wgm 'l. As a complement of the same pericope, we find 11 (eleven) mentions of hbb, viz. in CIS 96 (thrice; cf. below), 537, 1142, 1688, 2099, 3095, 3607, 4826 and 5376.

(a) Even taking the number of 11 as certain and adding to it the five mentions of hbb found in parallel pericopes, viz. CIS 1777: [w]gd sfr hbb;

CIS 23, 2170 and 4864: ng' 'l hbb; CIS 2489 is just next to CIS 2486 of the genealogical tree; and

CIS 99: tâwq 'l hbb; CIS 100 refers to the same hbb,

we see that the material with 19 mentions of hbb easily accounts for the 16 mentions of the same name in the pericope wgm 'l and its parallels.

(b) If the actual count of the mentions of wgm 'l hbb is easy to do, the most important aspect of the problem, viz. the actual value of that count aimed at determining how many different hbb's are mentioned, cannot unfortunately be accurately assessed. It is indeed impossible to prove that none of the hbb's listed in the last eight texts is the same as one of the three hbb's of CIS 96 or to prove that the hbb's of the last eight texts are all different from each other or to prove that all the hbb's of those eight inscriptions are different from the three of CIS 96. The truth most likely lies somewhere inbetween, that the number of 11 has to be cut by half because all the texts come from the same general area of Zelaf which also provided the long genealogical tree containing two hbb's.

(c) It is now possible to return to the actual count made by Clark to the effect that hbb in the pericope wgm 'l hbb "is far more common than that of any other personal name in the same context" (cf. the text above, p. 102). The cornerstone of the preceding argument is the expression "far more common." It is greatly regretted that Clark should use such a vague expression. In fact, he failed to give the proportion he arrived at during his study of the question although it would have been so easy for him to do so in less than half a line. The reader cannot but wonder why Clark preferred to remain silent and nobody can figure out what he had in mind. At any rate, an expression, such as "far more common," suggests at least a proportion of 10 (and above) to 1; a proportion of 3 or 4 to 1 can hardly be called "far more common."

wgm 'l is followed by the name hbb about seven times (cf. above); ksţ in three texts (CIS 2189, 2280 and

2732);

nr in three texts (CIS 2446, 2588 and 2596); and
wrd in four texts (CIS 2772, 4748 (and 4749), 4759 and 4860).

Note on kst - Due to the very close proximity of the texts, each of the two following groups refers to one kst only, viz. CIS 2189, 2190, 2191, and 2280, 2281, 2282, 2283, 2284, 2285.

Note on nr - The pedigree of CIS 2446 contains also the name of nr's homonymous grandfather. — CIS 2588, 2608, 2609, 2615 and 2779 refer to one nr only because the name is listed in each text immediately before or after gfwn.

The preceding figures show how manifestly exaggerated is Clark's statement since the proportion vacillates around 2 to 1, a proportion which cannot justify his expression "far more common."

e - ClAS's study of a few texts.

(1) Clark's first paragraph of his study of texts is devoted to Wi 227 (p. 189). JaSVW (p. 164) and JaMAR, XIII (pp. 23-24) have already dealt with the text. It may be added here that the beginning of his "SIJ 227a" is "*l bṯ ḥ()[ḥ]r*" (the correction of "ṯ" to "ṯ" is demanded by his page [iii]), but the preceding reading becomes "*l bs ḥ[rnḥ]r*" in the 1983 Ann Arbor reprint.

What a pity that no remark is volunteered by Clark on the very strange word "nnhr" which he understands as a noun! This is all the more regrettable in that one can only marvel at what might mean a form derived from the inexistent 7th form of nhr of which the 1st form means "to snort, snore" (cf. LaAEL, p. 2777A).

(2) Clark's second paragraph deals with LP 1231. He claims that the text

[A] " is listed in the Vocabulary by Littmann (LP, p.313) as
[B] containing an example of ḥbb as a common noun. This example should be deleted as the text in question does not contain this word. "

Ad [A] - This statement is inaccurate because LiSI lists it under the label "ḥbb subst. 'a friend' or n.pr. Habīb" (p. 313B).

Ad [B] - This statement is correct but it should be noted that LiSI was correct in listing LP 1231 under the label because the last phrase of his commentary suggests that "the last word is perhaps to be read ḥbb Habīb" (p.271). That section of the facsimile is very poor as the ʿ of ʿl after wgm is missing.

(3) Clark's third remark on ḥbb instead of "ḥbbb" of LiSI (p. 282) in LP 1285 is correct.

(4) Clark's fourth remark is to the effect that the 13 following texts, CIS " 23, 96, 1047, 1494, 1777, 1822, 2099, 2292, 3095, 3607, 4826 and 5376 all contain the word ḥbb in circumstances which suggest that it should be read as 'friend, loved one' rather than as a personal name. "

(a) Probably to confirm his thesis, Clark immediately adds that " The inscriptions C 304 and 305 have the feminine form ḥbbth, 'his beloved' (see JaSI, p.346). "

These two texts do not at all prove his thesis. The pericope, wwgm ʿl bdrt/ʿr ḥbbth rgmt mny, in which ḥbbth (with the personal pronoun -h) is apposed to a feminine personal name, is only another example of the well-known apposition case as also, e.g., ḥlh in wgm ʿl rfḏ ḥlh of Wi 358.

(b) The so-called "circumstances, which suggest" Clark's thesis, according to him, are nowhere described; they are not even summarized. In fact, they do not exist.

[1] Nine of the 13 texts listed by Clark are not even the objects of any remark in his presentation, viz. CIS 23: wngʿ ʿl ḥbb and he was afflicted because of Habīb;
CIS 1494: wslḥ ḥbb and he armed Habīb;
CIS 1777: w[w]gd sfr ḥbb and he [fo]und the inscription of Habīb (compare above, the LP texts on p. 99);
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CIS 2292, cf. above, p. 106; and
CIS 3095: wwgm ʿl ḥbb wʿl ʾšyʿh and he mourned over Habīb and over his companions.

Where are the famous "circumstances, which suggest" Clark's thesis? In fact, the most important of all those texts is the last one, CIS 3095, which clearly shows that, were ḥbb a substantive as ʾšyʿ, it too would have the personal pronoun as ʾšyʿ has.

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There is no reason to consider ʾtr as a plural, and Clark does not give any. Here again, where are those famous "circumstances, which suggest" Clark's thesis? The pericope may be translated as followed: and he was angry because of the trade of Ḥabîb. - For LP 475, where Clark claims that the verb ġḍb may occur, cf. JaMAR, XIII, p. 195.

== CIS "1822 and 2489 together with LP 398 and possibly LP 359" are dealt with together by Clark (pp. 189-190). For these texts, cf. above, pp. 100 and 101.

== Even in the final pericope of CIS 96, wwgm ʿl ḥbb wʿl ḥbb wʿl ḥbb, CIS states that ḥbb is "nomen proprium tres distinctas personas designans" (p. 208). It is with the discussion of this pericope (p. 190) that ClAS ends his long excursus on ḥbb, which is the longest section of his study devoted to a single expression. He summarizes the situation as follows: " The explanation offered by the Corpus editor for this most unusual expression is rather unlikely."

Here again, Clark does not justify his two opinions which remain mere statements. But, what is "most unusual" for a man to have three homonymous friends or for three homonymous persons to be mentioned in a single text? Why does Clark not inform his reader that the words wgm ʿl is followed by two frʾ's in CIS 103;
two ġt's in CIS 1936;
two ʾys's in WiTh 81; and
two tym's in CIS 5050?

It is also very interesting to point out that the same text CIS 1936 contains t w o sets of t h r e e homonymous persons each, viz.

three ġts's, viz. the author's grandfather in the pedigree, the author's brother and a man having no family relationship with the two others; and

three skrn's, viz. the author of the text, his grandfather and a man unrelated to either one.

Furthermore, the presence of several homonymous persons unrelated to each other is not "most unusual" in small communities. In the immediate vicinity of my own home, there were no less than eight "Emile," and at the present time, a Saf inscription could be written with the verb wgm followed by SIX ʿl's each of them introducing the very same name "Emile." It might be "most unusual" for the three homonymous ḥbb's to die at the same time or almost at the same time. But, since the author does not give the slightest information on the time of their death, it

is unreasonable to imagine a "most unusual" situation for the sake of a thesis. If such is the case, where is "this most unusual expression" and how could CIS's interpretation be "rather unlikely"? Finally, Clark does not refer to yet two other pericopes which should have been mentioned in his discussion of the subject, viz. (wg)[m] ʿl ʾḫh trḥ wʿl ʾḫh in CIS 2526 (cf. my paper in Orientalia, 36 [1967], p. 346), and

"wsfr lḫlh wsfr lḫlh" in Wi 291 according to WiSIJ, p. 50.

WiSIJ translates the latter as follows: "And he wrote for his maternal uncle, and he wrote for his true friend," with the comment that "if the repetition of wsfr lḫlh is deliberate, a play on the double meaning of ḫl ... must be intended."

There is no reason for doubting the genuity of the second expression since the palaeography is the same in the whole text. Furthermore, the meaning of ḫl as "maternal uncle" is so well documented and so constant that Winnett advocated his second meaning in one text only, the present one. His reason for doing so is exclusively the repetion of the expression. Such a reason is contradicted by CIS 2526 where ʾḫ retains its meaning (what else could it mean?) in each expression. Besides, what is so exceptional for a man to have two "maternal uncles"? Moreover, Winnett's argument is futile because his decipherment of the text omits one letter, n before the second ḫlh. Finally, ḫlh was already known as a personal name in CIS 667 (lḫlh bn ḥkr; cf. JaSN, p. 64, note 96), and since then, Winnett added another reference in WH 469 (lḫlh bn ʾnʿm ...). Therefore, the pericope of Wi 291 reads as follows: wsfr lḫlh wsfr lnḫlh And he wrote for Ḫâluh, and he wrote for Naḫluh. — nḫlh, cf., e.g., nḫl in CIS 622. — The author of Wi 291 wrote for two different persons. In CIS 2526, the two ʾḫ's evidently refer to two brothers of the author. The sole difference between CIS 96 and 2526 is that ḥbb is repeated thrice in the first, and ʾḫh is repeated twice in the second. The interpretation based on the Saf material cannot be altered because an expression occurs twice or thrice in succession. As seen above, Winnett sought to understand the repetition of an expression in the twofold meaning of ḫl contrary to the Saf material.

Clark also sought the solution in another direction, but also outside the Saf material. Indeed, his thesis is presented as follows:

" In fact the word ḥbb is probably thrice repeated in order to emphasise the affection of the author of the text for his friend and could be read as "a friend three times over"." (p. 189).

The basic characteristic of the usage of the verb wgm, however, is that EACH person or EACH group of persons who are the objects of the action specified by the

verb is introduced by the preposition ʿl, and CIS's above-mentioned statement is the simple wording of that well-known epigraphical fact. Clark's opinion, on the contrary, which rejects the individuality of each person introduced by ʿl, is the clear denial of that epigraphical fact.

Summarily, instead of containing even a single shred of evidence in support of Clark's opinion, the Saf material shows an overwhelming epigraphical information that Clark had to ignore in order for him to formulate his thesis. His distortion and denial of the epigraphical evidence cannot in any way be excused or made excusable by his endorsement of Littmann's, Winnett's and Oxtoby's position in the matter, for they too had ignored and distorted the same epigraphical evidence. It is deeply regrettable that Clark learned so well from them how to so crookedly manipulate the Saf material. Depending on the kind of pericope used in the texts, hbb is a personal name (cf. CIS's statement), or a verb, or a noun in the status constructus the friend of.

2 - On the second name mskn (cf. remark above, p. 85), the author notes that

" it is possible that the small horizontal stroke, read here as *n*, is not a part of the name, as the other examples of the letter *n* in this text are vertical strokes; this would give the well-known name MSK."(p.17).

First of all, instead of the expression "vertical strokes," which is correct in n of the last bn only, it would have been accurate to speak of the straight line of n being perpendicular to the line axis. Yet, even then, his remark would still be valueless for the reading of the letters; e.g., even in his ClNSI 2 the n's are either in, or aslant to, the letters axis. Furthermore, why should Clark make such a remark? If he believes in his above-quoted basic principle of reading on "all the variant forms ... can be explained as accidents of engraving," he should have even less difficulty admitting that the straight line of n may be engraved either vertically or aslant or horizontally with regard to the line axis. Finally, here again, Clark does not care to look in the texts he himself published in his thesis to see whether or not the same feature does not occur. Had he done it he would have found that the straight line of n is, e.g.,

in the horizontal axis in ClAS 77, 1004 (FOUR TIMES) and 1007;

in the axis of the lower edge of the letter line in ClAS 549; and

aslant to the line axis in ClAS 103, 130, 154, etc.

Not a single commentary on the above-listed inscriptions makes the slightest remark on the case at hand. Clark could also have found more examples in other collections of Saf texts.

3 - The end of ClNSI 4 is read by the editor as follows:

"[*b.r*] is the [..]". " (p. 17).

a - The first section of Clark's commentary reads as follows:

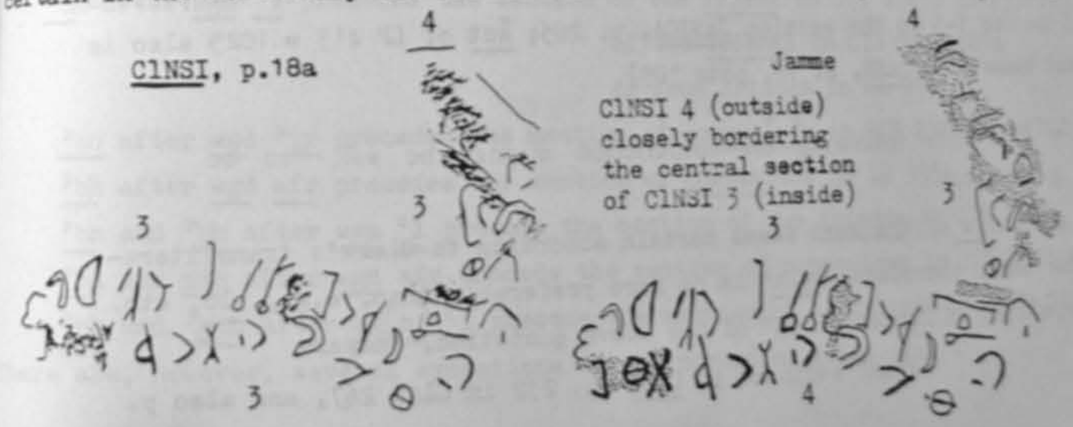
"The final word of the text, which has been damaged, may read *b'r*, "the ass", although this could hardly refer to the animal in the accompanying picture, which appears to be a horse. " (p. 17).

(1) The first part of the statement suggests the possible reading of "hʿr, 'the ass'." But, since the editor declares himself unable to read the central letter, his suggested restoration is no more probable than, e.g., hwr to be debilitated of CIS 204, or hyr to prostrate of wH 1772, or hkr to be greatly startled of CIS 2292, or hrr disgust, or anything else. (2) The second half of the same statement relating to "the accompanying picture, which appears to be a horse," is irrelevant as stated at the end of my discussion of ClNSI 3 (cf. above, pp. 96-97). It is also arbitrary since there is no reason why Clark should eliminate the mention of the animal on the left; neither figure is related to the text.

b - The end of Clark's commentary on the last word states that

" Both the *b* and the *r* can be made out on the stone; there may be one or two other letters between but these are totally illegible. " (pp. 17 and 19).

Clark's confusing reasoning method is once more exemplified here: the "h" is certain in the commentary, restored in the transliteration; and the translation considers it in all certainty as the article; this suggestion is based exclusively on his gratuitous and, even according to him, hypothetical reading hʿr; the "r" is certain in the commentary but restored in the transliteration; and the possibi-



lity of two "totally illegible" letters is certain in the commentary but is excluded by both the transliteration and the translation; it also obviously contradicts his suggested reading of "hᶜr."

4 - The text ClNSI 4 may be read as follows:

lmskn bn mskn bn wsm bn ʾbd hwr

By Maskân, son of Maskân, son of Wasîm, son of ʾAbd. He became weak [here].

For the reading of the three central names, cf. the edition. — hwr, cf. hr, e.g., ClAS 1192 (cf. JaMAR, XIII, p. 112).

F - ClNSI 5, 6 and 7 (p. 19, facsimile on p. 18b, and photograph on pl. 2b).

These three texts engraved on the fourth stone are too difficult to read due to the dull grayness of the photograph. It is to be hoped that a clear photograph will be published so that the inscriptions may be accurately studied and their reading ascertained.

1 - ClNSI 5 - For Clark's translation of the verb syr as "he returned to the encampment," cf. above, p. 96.

2 - ClNSI 6 - The commentary contains two statements which must be taken up here.

a -

" ŠDT is known only as a tribal name, in WH 2005 and JaS 3-5 ." Clark's opinion on JaS 3-5 is copied from WiHaI's commentary on WH 2005, viz. "the tribe of ŠDT is mentioned in JaS 3-5" (p. 310). In these three texts, šdt obviously is a personal name: lmḥsm bn šdt bn mḥlm of JaS 3;

lmsk bn šdt bn mḥlm bn ... of JaS 4; and

... mḥlm bn šdt bn mḥlm of JaS 5 (cf. below, my study of Winnett's recent paper). Cf. also the genealogical tree of the persons involved in JaS 3-5 in the edition JaSICA, p. 203; šdt of LP 413 = 1025 also is a personal name (cf. JaSN, p. 76, note 109).

b -

" The verb following the conjunction *w* may be *wlh*, "to be griefstriken".

Only the initial w of the verb seems certain according to Clark's transliteration of the text; therefore, "wlh" is no more preferable than, e.g., wld, etc. With the spelling of "griefstiken" of the above quotation, compare

"grief striken" in ClAS (p. 232 in ClAS 267, and also p. 476) and

"grief-striken" (cf. l.c., p. 404 in ClAS 1010).

Thus, three different orthographs having in common the very same misspelling, viz. the absence of "c." For the well-known "grief-stricken," cf., e.g., A Supplement to the Oxford English Dictionary, Oxford, I (1972), p. 1298a, under "grief."

G - Appendix - The nouns of family relationship and their order of precedence in the pericope of verbs expressing feelings.

The verbs expressing feelings, viz. wgm ᶜl, wlh ᶜl, ndm ᶜl and tšwq ᶜl are very well-known, but tql ᶜl is attested only in LP 306. To them wgd ʾtr and wgd sfr may be added because of the feelings which are understood as the direct effect of the material act of finding the inscription or the trace of a beloved one. It may safely be affirmed that the word "trace" actually means text, viz the same reality considered from two different view points.

Many of those pericopes are followed by a single noun; e.g., wgm ᶜl ʾbh if CIS 303; wgm ᶜl ʾhh of CIS 845; and wgm ᶜl ʾmh of JaS 70. The meaning of the verb demands that each text be engraved after the death of the person mentioned.

1 - In many other inscriptions, the verbal expression introduces a list of several persons. The most important element for the understanding of those lists is the nature of the circumstances in which they were written. Unfortunately, they will always remain unknown, and there is nothing to do but to register those lists as they are. Those lists show an order of precedence which, although less precise and complete than we would like to find, is nonetheless valuable and suggests that any disturbance of that precedence may be explained by special circumstances which a specific author was facing when writing his text.

In those texts, the mention of the author's relatives precedes that of friends and companions; e.g.,

ʾbh after wgm ᶜl precedes the mention of one friend in CIS 4765, IM 23917a;
of two friends in CIS 83, 1689;
of four friends in Wi 991;

ʾbh after wgd ʾtr precedes the mention of companions in CIS 2546 and 2722;

ʾbh after wgd sfr precedes the mention of campanions in LP 395;

ʾbh and ʾhh after wgm ᶜl precede the mention of two friends in CIS 4273;

ʾbh and ddh after wgd sfr precede the mention of companions in CIS 2547; and

ʾhh and ʾhth after wg[m] ᶜl precede the mention of 11 friends in WiTh 81.

There are, however, several exceptions to the preceding rule; viz.

hlh is preceded by *brd* after *wgm ʿl* of CIS 2833;
hlh is preceded by *qhs* after *wgm ʿl* of WH 593;
ʾmh is preceded by *bnyt* after *wgm ʿl* of CIS 5044 (cf. above, pp.99-100);
ʾhh is preceded by *ʾys* after *wgm ʿl* of WH 1030; and
ʾbh is preceded by 10 persons after *wgm ʿl* of J 13948.

The precedence of relatives upon non-relatives is of about the proportion of 2 to 1.

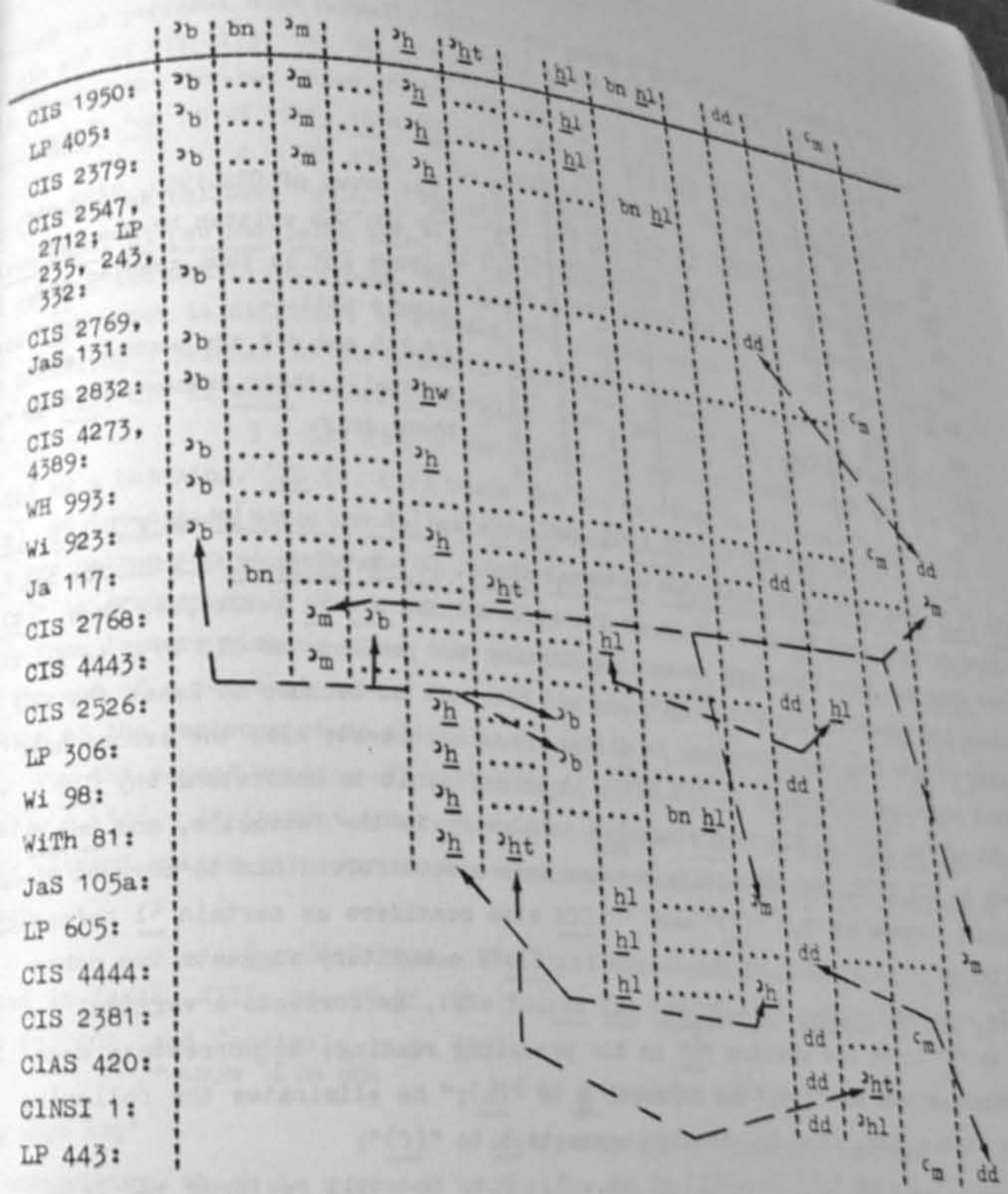
2 - The texts where the verbal form expressing feelings is followed by noun(s) describing family relation(s) with the author are best presented in a schematic manner. The schema and two complementary additions are reproduced on pp.117-118. The importance and precedence given to the male relatives of the author are overwhelming and especially in the case of both the "father" and his brother(s), the "paternal uncle(s);" these two must have been the backbone of the social family hierarchy. The mention of the "grandfather" (less than 1/3 of that of the "father") may be explained by the fact that the grandfather was normally dead before his grandson was a mature adult and, thus, before he could have had much, if any, personal relation with his ancestor. Then, comes the mention of the "brother" (more than 1/2 of that of the "father"), followed by that of the "maternal uncle" (more than 1/3 of that of the "father"). Only then, comes the mention of the "mother"(1/4 of that of the "father") immediately followed by that of the "sister" (less than 1/5 of that of the "father"). The secondary importance of the "mother" in this strongly male-oriented society is evidenced by the number of texts where she and the male relatives of the author are listed first, viz. *ʾm* 2 ⟷ *dd* and *hl*: 3 each;
ʾh: 4; and
ʾb: 13.

Here follow notes on 14 texts mentioned in the preceding lines, viz. CIS 1950, 2379, 2768, 4988; ClAS 1004; IM 23917a; JaS 117; LP 135, 820, 1101; Ox 57, 156; Wi 471 and WiTh 81.

3 - CIS 1950 and 2379 (cf. CIS, pp. 158 and 310, respectively). The two central pericopes of the texts and the following word are reproduced here as in CIS:

CIS 1950: "*wwgm ʿl ʾbh wʿl ʾmh wʿl ʾhh wʿl ḏbn (ʾ)hh hbʿlsmn*"
CIS 2379: "*ww[g]m ʿl ʾbh wʿl ʾmh wʿl ʾhh wʿ[l] ʾws wʿl bn hlh wġwr*"

JaSVW has suggested that "the copy of CIS 1950 had telescoped the end by forgetting one element of the series, and *(ʾws) [wʿl] hlh* is to be read" (p.



Verbal expressions used in the inscriptions:

wgd ʾtr: WH 993; *wgd sfr*: CIS 2547, 2769; LP 443;
wgm ʿl: CIS 1950, 2379, 2381, 2526, 2712, 4273, 4389, 4443, 4444; ClAS 420; JaS 105a, 117, 131; LP 405, 605; Wi 923; WiTh 81;
wgm ʿl and *tšwq ʾl*: LP 235, 243; *wlh ʿl*: CIS 2768; LP 332;
ndm ʿl: Wi 98; *tšwq ʾl*: CIS 2832; ClNSI 1; *tql ʿl*: LP 306.

	1st place	2nd place	3rd place	4th place	total
ʾb	13	3	-	-	16
ʾhl	-	1	-	-	1
ʾḫ	4	4	1	-	9
ʾḫt	-	3	-	-	3
ʾm	2	2	1	1	6
bn	1	-	-	-	1
bn ḫl	-	1	-	1	2
dd	3	8	3	-	14
ḫl	3	-	2	1	6
ʿm	1	4	-	-	5

The group of CIS 1950, 2379 and LP 405 was written by the same person and is thus counted as a single unit.

LP 235 and 243 are counted separately; their authors are brothers only.

160) at the end of CIS 1950. But, the end of the copy shows wʿl lnl ḫlh [ḥ]bʿlsmn and over Lînil, his maternal uncle. [O] Baʿalsaman. As such the pericope gives a good meaning and is in the style of the wgm ʿl pericopes; thus, there does not seem to be any reason for forcing the pericope of CIS 1950 to reproduce the phrasaeology of CIS 2379 and all the more so because at least the beginning of the following pericope is different in each text; also the second half of that pericope is missing in CIS 1950. It is difficult to understand why CIS thought of "bn ʾḫh" in CIS 1950 since ḫlh is already in the facsimile, and bn ḫlh already in CIS 2379, and especially because such a move forced him to correct three horizontal strokes as "b̊," "(ʾ)" and "b." CIS also considers as certain ʿl before ʾḫh although it is missing in the facsimile. CIS's commentary suggests two other possible readings: either "wʿl bn(yḥ) (f) ḥb..." But, he corrects a vertical stroke to "b" which was written "b̊" in his preceding reading; he corrects a second vertical stroke as "(y);" he corrects ḥ to "(ḥ);" he eliminates the following vertical stroke; and, finally, he also corrects ḥ to "(f)";

or "wʿl ln w[ʿl] ḫl ḥb...". But, he still restores two letters and should have written "[wʿ]l" instead of his "w[ʿl]." It is not possible to establish the chronological order of CIS 1950, 2379 and LP 705 who have the same author in common; no clue is provided for by the texts which have, respectively, four, five and six names in the pedigree and also have a slightly different end of the pericope: ... w[ʿl] ʾḫh wʿl lnl ḫlh in CIS 1950;

... wʿl ʾḫh wʿl ḫlh wʿl ʾws in LP 405; and

... wʿl ʾḫh wʿ[l] ʾws wʿl bn ḫlh in CIS 2379.

Although the personal name normally follows the noun+ḫ in similar pericopes (e.g., ḫlh sbʿ of CIS 1275, ʾḫh ʿwd of CIS 2832, and ʿmḫ sbʿy of CIS 3140), the formula showing the reverse order of the words, as here, lnl ḫlh, is also attested (e.g., grmʾl ʿmḫ of WH 1681, ʾnʿm ʾḫh of Wi 98, and rfd ḫlh of Wi 358).

4 - CIS 2768 - The first pericope of the text is read by CIS (p. 361) as follows: "wwlh ʿl ʾmh w[ʿ]l ʾ̊bh wʿl ḫlh Et lamentatus est super matrem suam, et [su]per patem suum, et super avunculum suum." The facsimile of the central section is very poor, but lʿ is clear above the hatching where CIS reads "ʾ̊b" which is difficult to explain. The passage reads lʿḥ which, alongwith the preceding w belongs to the mother's name, wlʿḥ Walaʿuh, instead of "w[ʿ]l ʾ̊bh" of CIS. Cf. Ar walaʿ "desire, longing, love, passion" (cf. SaAAL, p. 1258).

5 - CIS 4988 - The centers of two contiguous loops are affected by a hatching. CIS (p. 612) reads the two sections as follows: "wwgd s[fr] dd[ḥ] Et invenit in[scriptionem] avi [sui]" and "wwgm ʿl ʿ[mḫ w]ḥbḥ ... Et posuit pro patruo suo et amico suo ..." But, "d" of "wgd" is certain. The "s" of "s[fr]" is erroneous; if the letter is correctly traced, it is g. There are probably four letters missing after the above-mentioned g, and "dd" is most probably the end of a personal name. There is no room for "[ḥ]"after dd. The first section of the pericope reads as follows: wwgd g.. ...dd And he found .. of...dâd. For the second section of the pericope, cf. my paper in Orientalia, 36 (1967), p. 346. It is most interesting to note that CIS very surprisingly accepts without making any comment the erroneous absence of ʿl before a complement of wgm.

6 - ClAS 1004 - The first three pericopes of the text are discussed in JaMAR, XIII, pp. 89-90. The fourth and last one is read as follows by ClAS (p. 402, and facsimile on p. 579):

" *wwjm ʿl bn dd*ḥ — And he grieved for the son of his paternal

msby sbyt hty' — uncle, whom the TY' took as a captive."

The commentary, which is supposed to justify the translation, is so foreign to it that it is difficult to reconcile the two together. Since the editor considers msby as a "part." (p. 474), it should have been translated "being captive;" in fact, it becomes "whom" in the translation. The word msby is a noun having the the form of the passive participle.

Furthermore, the editor considers sbyt as "n. captive" (p. 472) and declares that

" In the expression *msby sbyt* the word *sbyt* is an absolute

object of the participle *msby* (for which see the expression

msby ḫwlt in HCH 132)."

Thus, his translation should have been something like this: "captive of the captive of the TY'." In fact, msby ḫwlt *of HaCH 132 is a nominal apposition to* ʾḫh *which immediately precedes the expression (cf. above, p. 87), and* sbyt *is the feminine of the verb* sby *to capture having as subject* ḥṭyʾ*, and* sbyt ḥṭyʾ *is an asyndetic relative proposition; cf., e.g., as in* wgm ʿl šrk qtl wʿl ʿyd sby wʿl ʾnʿm qtl *(CIS 2113), which may be translated as follows:* he mourned over Šarik [who] was killed, and over ʿÂyid [who] was captured, and over ʾAnʿam [who] was killed, *and also* wgm ʿl ḫlh ʿlm fny dʾl df *of J 13212* and he mourned over his maternal uncle ʿÂlim - [who] passed away -, him of the tribe Dâf *(cf. JaMAR, XIII, pp. 149-150). In an effort to justify his understanding of* sbyt*, Clark states this:*

" For the word sbyt see C 1361, the final pericope of which reads wwjm ʿl ʿ-šḥ sbyt qbr, which seems to mean "and he grieved for his companion, captive of a tomb"." (p. 404).

This translation, which incorporates an expression as non-Saf as "captive of a tomb," is erroneous. CIS correctly translates the complement of wgm ʿl as "socio suo Sabbîato. Sepultus est" (p. 183) with the correction that qbr is another asyndetic relative phrase meaning [who] has been entombed.

The pericope of ClAS 1004 may be translated: And he mourned over the son of his paternal uncle, the captive [whom] Haṭayʾ captured.

In his text immediately preceding his argumentation (pp. 403-404), Clark refers to Saf ClSN 1b = J 14200b and repeats his previous reading of the text in Abr-Nahrain, 17 (1976-1977), p. 37: "ww[jm ʿl ..]b bn ʾḫh mn qtl and he [grieved for ..]B, the son of his brother who was killed." Incidentally, as shown by the blank toward the bottom of p. 403 [here follows the reproduction of the bottom of the page in the 1983 Ann Arbor printing:

" noun comparable to the Ar. qatl or qātil, "killing, murder".

See also the expression ww[jm ʿl ..]b bn ʾḫh mn qtl in CNAI 1b, which was translated as "and he [grieved for ..]B, the son of "] the two lines immediately above the preceding quotation in the thesis were already supposed to be scratched out according to p. "[vii]." After learning in a

private correspondence" from A.F.L. Beeston that Saf mn does not correspond to Ar man, Clark altered his translation and states that "the last two words of this must be read as 'from being killed'." Beside rejecting his original interpretation of mn, JaMAR, VIII, p. 13, points out that his "ḥ" and "mn" must be corrected to]r and ml, respectively. The translation of the pericope with ww[gm ʿl] reads a follows: And [he] has mo[urned over ...]ar, son of his brother Wall [who] was killed.

7 - IM 23917a - For the reading of the last word, cf. my paper in Sumer, 25 (1969), p. 142A.

8 - JaS 117 - This text was independently published by Y.M. Abdallah, an-Nuqûš aṣ-ṣafawîyat ... (cf. above, p. 103), p. 96, and facsimile on p. 93, under the siglum [AbNS] 43. The first word depending on wgm ʿl is "ʾḫh" in the edition (cf. also p. 240), but "bnh" on the facsimile.

9 - LP 135 - The last pericope of the text is read by LiSI(p. 27) as follows: "wwgd sfr whbʾl hll And he found the inscription of Wahabʾêl, a friend." The Saf material makes it certain that, were the editor's interpretation accurate, the personal pronoun would be affixed to hll: whbʾl would not, and could not, be just "a friend," he would have been "his [= the author's] friend." But, the editor adds that "the last word should perhaps be read hlh, 'his maternal uncle'." The last letter is clear in the facsimile and cannot be changed without a most compelling reason. Such a reason does not exist because hll gives a very acceptable meaning when understood as a verb (e.g., JaS 119a and JaSN, p. 74, note 103) of an asyndetic phrase. The pericope may be translated as follows: "and he found the inscription of Wahabʾil [with whom] he associated as a friend."

10 - LP 820 - The text is read by LiSI as follows: "lš(ʿ)ʾl wʾḫ(h) By Shai(ʿ)-ʾêl and his brother." The commentary makes certain that the two sets of parentheses have two different meanings, viz. the first indicates the restoration of a letter, but the second a simple correction. Neither change is justified. Furthermore, the editor's interpretation is built upon an unacceptable feature, viz. a text having two authors; cf. also below, Winnett in Wi 471. The text may be read as follows: lšʾ lwʾ hʾ By Šayʾ. Hawʾ was ugly.

šʾ, e.g., WH 2139. — lwʾ, cf. Ar lawaʾa "to be foul, ugly, or deformed in countenance" (cf. LaAEL, p. 2677A). — hʾ, cf. Ar hawá (i) "to be, become, hungry" (cf. l.c., p. 827B).

11 - LP 1101 - The pericope of the text is read by LiSI (p.

249) as follows: "ww(gm) ʿl ʾḫ(h w)ʿl ḥn wʿl ʿm And he laid stones on the tombs of his brother and of Ḥunn and of ʿAmm." Cf. already above, my remark on p. 99. - The two letters gm are restored and should be put between brackets. The editor's reading is based on his gratuitous remark according to which "the order of h and w in ʾḫh wʿl has been inverted." Even at first sight, this remark is absolutely gratuitous and even more so since ʾḫwh makes good sense, his two brothers, and introduces two personal names. Furthermore, the editor reads a dot and an incomplete letter as ʿ's; the facimile is too poor to figure out what these letters actually are. The most plausible reading of the text is w[gm] ʿl ʾḫwh [.]lḥn w[.]lʿm He mo[urned] over his two brothers [.]alḥān and [.]alʿum.

12 - Ox 57 - The formula ʾḫthw his sister is also found in the present text according to HaAIC's correct mention of the feminine name ʿḏb (p.411). This ʿḏb is the sister of the author of the text, ʾḫth ʿḏb, instead of "ʾḫt hʿḏb the sister of Ha-ʿAdhab" of the edition (cf. OxSI, p. 48, and facsimile on pl.4). The name ʿḏb is also known as a masculine personal name, e.g., in Wi 257.

13 - Ox 156 - The text is read by the editor as "lʾdb bn ml hʾb By Adīb b. Mayl, the father" (cf. OxSI, p. 66, and facsimile on pl. 6). The statement of the translation is unacceptable because it involves a novelty which is rejected by Saf phrasaeology; it involves an obvious reduplication. Furthermore, the editor's first "b" is r (cf. JaROx, p. 282), and the third "b" is r because of the form of b of bn. The text may be read as follows: lʾdḥ bn mlh ʾr By ʾAdaḥḥ, son of Malūh. He had intercourse [here]. — ʾdḥ, cf., e.g., dḥ in LP 789. - mlh, e.g., CIS 1183. — ʾr, cf. Ar ʾarra ou ʾāra (i) "inire, to compress (a woman)" (cf. LaAEL, pp. 44C and 136C, respectively).

14 - Wi 471 - The text is read by WiSIJ (p. 72) as follows: "lʾḏnt bn rfʾt wʾḫh By ʾUdhainat b. Rafʾat and his brother." The editor indulges into two errors, viz. he interprets ʾḏnt as a qutayl nominal derivation and considers the text as being written by two persons (cf. above, p. 121, Littmann in LP 820). The first opinion was denounced in JaROx, p. 280, and the second in JaSN, pp. 97-98. Furthermore, the editor reads the ninth letter, ġ, as "f," the tenth letter, h, as "ʾ." The text may be read as follows: lʾḏnt bn rġ htw ʾḫh By ʾAḏnat, son of Rawġ. His brother was trampled on [here]. — rġ, cf. ʾrġ in ClAS 638. — htw, cf. Ar hatā (o) "to stamp, trample on" (cf. SaAAL, p. 1132B); here, in the passive voice.

15 - WiTh 81 - To my remarks in JaRWiReAR, p. 525A, add that the name of the first friend is ṣlt Ṣallat, and also that the photograph is printed upside down compared with the facsimile.

VI - F.V. Winnett on Safaitic and Thamudic ʾtm and ʾtm ʿl.

A - Introduction. - In the last issue of JAOS, 117 (1987), pp. 239-244, Winnett submits his views on Saf and Tham ʾtm and ʾtm ʿl. The dates 1937 and 1987 seem to obvious, however, to be the result of blind luck since his last paper appeared in 1981; cf. "A Reconsideration of Some Inscriptions from the Taymāʾ Area" in Sources for the History of Arabia, Part I, Royad, 1399/1979 (released in 1981), pp. 69-78. With the exception of the two plates (pp. 78 and 89), this paper develops the same ideas as in "A Reconsideration of Some Inscriptions from the Tayma Area" in PSAS, 10 (1980), pp. 133-140. My answer to the first edition is in JaMAR, XIII (1983), pp. 176-182. It does not seem presumptuous to think that Winnett planned on quietly celebrating the golden jubilee of his WiSLTI published in 1937 by producing a paper on Saf and Tham. Celebration or not, I cannot but deplore the quality of the article printed in JAOS. Indeed, the author actually outdid himself in concentrating in so few pages so many errors, arguments for the sake of the cause, contradictions and nonsense talks.

B - Summary of my theory and Winnett's.

In order to help the reader to more easily see his way through the maze of the following pages which follow Winnett's exposé, it seems useful to summarize here my theory and Winnett's paper.

1 - My position on ʾtm may be presented as follows.

a - The normal and most obvious etymology of Saf/Tham ʾtm is Ar ʾatama.

b - ʾtm is used without any preposition in the sexual context of Saf CIS 285 and also in JaS 3. My translation of ʾtm without any preposition as to engage in sexual relations is based on both the context of CIS 285 and the basic meaning of Ar ʾatama to gather together.

c - When the verb ʾtm is used with the preposition ʿl (e.g., JaS 2), mindful of the possible real impact of the preposition upon the verb, I translate the expression ʾtm ʿl as to come together with because of the basic meaning of Ar ʾatama. Would a still unknown text with the same verb contain a sexual context, my translation could easily be adapted to the context by the mere addition of the adverb "sexually" to my translation of to gather together.

d - Tham being close to Saf not only geographically but also linguistically, I transfer to Tham my translation of Saf ʾtm as to engage in sexual relations.

e - This suggestion on Tham ʾtm is confirmed by no less than three inscriptions, Hu 104, 263 and 317 for the context of each of them contains the verb wdd to love.

2 - Winnett's theory follows a winding path with quite unexpected and baffling meanders.

Saf. a - He accepts the sexual context of CIS 285. But, on the basis of one of his favorite ploys, viz. "the omission of ʿl in JaS 3 is probably the result of an oversight on the part of the writer" (p. 239B), he claims that either ʾtm or ʾtm ʿl "probably means 'he mourned for'" (p.239B) although it is a well-known fact that all Saf verbs expressing feelings, e.g., wgm ʿl, tšwq ʾl, etc., govern their objects through a preposition.

b - In spite of his claim to adhere to the basic meaning of Ar ʾatama, he actually chooses the second of the secondary meanings of the root.

c - He does not even mention both V.A. Clark's and A. van den Branden's opinions on Saf ʾtm.

Tham. a - Due to his fallacious theory on the imperviousness of his class "B" to which he assigns ALL ʾtm texts (p. 239B), he states that "ʾtm in Thamudic B has a different meaning from what it has in Safaitic or in late Thamudic texts" (p. 240A).

Such is his thesis; but, instead of proving it, he translates the texts accordingly and, of course, in his opinion, his translations prove his thesis. However, since he assigns ALL ʾtm texts to his "B," his "late Thamudic texts" do not exist.

b - Because of another fallacious assumption according to which in a text beginning "with the invocation of a god" "the verbal form which follows the divine name is almost of necessity an imperative" (p. 240A).

But, the calling upon a god often is a mere exclamation of surprise, admiration, oath, etc and not an invocation for help, and such an exclamation does not have to be followed by a verb or a verb in the imperative mood. It is an obviously wrong assumption that such exclamation could not introduce a name or an ordinary

phrase (affirmative or negative) or a simple substantive.

c - Then -it should be emphasized that winnett deals with Tham - he rejects V.A. Clark's theory in Saf so cavalierly that the latter's opinion is scamped in three short lines.

d - After that, A. van den Branden's translation of Tham ʾtm as "rends parfait" is rejected because of an unbelievable reason: the expression "rends parfait" = make perfect, according to Winnett's own written testimony, "suggests a striving after moral perfection" (p. 240A). The blinding power of Winnett's preconcieved theory on what ʾtm should mean had to be devastatingly effective to prevent him from seeing how preposterous his statement really is.

e - Then, since he had burned all bridge with "Safaitic and . late Thamudic texts," since he rejected without giving his reader any reason the obvious validity of the parallel Ar root ʾatama, he borrowed E. Littmann's etymology of ʾtm from the root tmm (endorsed also by both van den Branden and Clark) which, in turn, led him to the technical Hebrew tamîm, an adjective describing the physical appearance to be met by all animals before being slaughtered in the temple of Jerusalem. On such a basis, he decided that ʾtm had to mean "to restore to health." But, the Hebrew adjective directly refers only to the external appearance of those animals and not to their actual health, and the Tham texts do not speak either of animals, or slaughter or temple. Since they speak of human beings and do not contain the slightest indication either that any of those men was sick or that his physical appearance left much or even little to be desired, it is unthinkable of any reasonable human being to ask a god "to restore to health" any of those persons. Yet, according to Winnett, "when men call upon the gods, it is for the purpose of asking some boon" (p. 240A). Where is the "boon" to be asked and expected since each of those men already has it? But, since the texts are there and cannot be ignored, a possibility of explaining their existence would be for Winnett to invoke one of his remarks, viz. each text "is probably the result of an oversight on the part of the writer"(p. 239B) unless, of course, each writer intended his text to be ... a big joke. Note also that the four authors having recourse to the root tmm presented each a different interpretation.

f - Moreover, Winnett attempts to discard the wdd confirmation of my opinion on ʾtm by disemboweling wdd and substituting its basic meaning of to love, lover with "to be fond of; friend."

g - The last general characteristic of Winnett's paper is that, in an apparent move to suit himself and to assure the smooth sailing of his arguments, he ignores some publications, such as JaMAR, IV, XIII, JaRWiReAR, JaSN and LiSI; ignores many inscriptions, including some he published himself, such as WH 374, 375, 376 and 377; and ignores some sections of other publications, such as JaMAR, V, and JaSISA.

In fact, however, Winnett's paper is the last example of a mind swinging with another temporary current of the wind. Indeed, he had already launched his Tham theory on ʾtm meaning "to restore to health" in 1978 when he stated in WiHaI (p. 434, on WH 3032) that ʾtm "occurs frequently in Tham. with the meaning of 'to restore to health'." But, he had then applied the same translation to his Saf WH 3032. Now and without even as much as a reference to his own translation of WH 3032, he swings again into another direction, "to mourn."

I know how boring it is to read over and over again the same or similar criticisms sounding like old broken records. Unfortunately, however, nothing else could be done for Winnett's argumenting method consists of a few predetermined ideas and ploys, including quiet self-contradiction, which are repeatedly put to use over and over again to saturation.

C - The Saf section of Winnett's paper.

1 - Winnett's initial paragraph devoted to Saf starts with these amazing statements:

> "M. C. A. Macdonald has drawn my attention to the fact that according to Lane the basic meaning of *ʾtm* is the tearing of the seams of a milk or water skin so that two small holes in the seam are joined together and become one. From this by extension it came to be applied to something very unpleasant happening to the female genitalia, namely the splitting of the skin between the vagina and rectum of a woman.[1]" (p.239A).

a - So, as phantastic as it may seem, Winnett never read LaAEL himself and a friend's initiative was needed to draw his "attention" to an information so readily available of which he should have been aware so long ago. It is also amazing (but is it really?) that Macdonald speak of "the basic meaning of ʾtm" on the sole basis of LaAEL; that Winnett endorses Macdonald's opinion in spite of the testimony of his most beloved Ar dictionary, SaAAL, who does not even mention such a meaning; cf. "Cut. - (b) [Bi], Dwelt in" (p. 3A); and, finally that neither Winnett nor Macdonald, apparently confident in their own acumen, did consult DAFA (I, [1967], p. 23), where the following statements are clearly printed:

أتم

A *Notion de base fournie par:*

مَأْتَمٌ *pl.* مَآتِمُ réunion de femmes (*qqf.* d'hommes) / gathering of women (*sometimes* of men) (*LA*; N) ||

réunion funèbre (de femmes) / funeral gathering (of women) (*LA*; N) || pleurs, lamentations; deuil / tears; mourning.

(p. 23A-B)

Furthermore, CoDRS (fasc. 1 [1970]) does not list a single meaning connected with sexuality, but concurs with DAFA; his entry reads as follows (p. 36B):

C I — s'élargir par déchirure (orifice) / to widen by tearing (orifice).

أَتُومٌ (femme) qui a la vulve déchirée / (woman) who has a torn vulva (*LA*; N).

II — déflorer, violer / to deflower, to ravish.

IV = II.

(p. 23B)

Any sexual meaning appears ONLY in the THIRD meaning, viz. "C I"

" 'TM, 1. AR. *'atima* «être lent»; *'atm-* «inertie». -?2. *'atama* «résider en un lieu»; *ma'tam-* «réunion funèbre de femmes»; SAR. *'tm* «réunier, associer»; soq. *'etom* «rassembler, réunir»; *'itim* «compagnon». -3. AR. *'atama* «s'élargir par déchirure (orifice)». -4. *ma'tamat-* «pilier(?)». -5. *'utm-* «oliviers sauvages»."

Therefore, both Macdonald's and Winnett's claim to have pinpointed the basic meaning of Ar ʾatama is erroneous.

b - Note 1 of Winnett's text refers to ClAS, "p. 156." In note 42 of that page, ClAS publishes the following statements:

" The word *'tm*, if from the root *'tm*, refers to the splitting of the skin between the vagina and rectum of a woman. By analogy it is used to mean "to come together (of an assembly of women)" and it may mean "to rip"."

Clark also limits his horizon exclusively to LaAEL whom he summarizes. The translation of ʾtm as "to rip" is also that of LiSI, p. 184 in LP 720: "to be ripped," a translation which becomes doubtful on p. 301A . Cf. already JaSISA, p. 200, on JaS 2. Thus, Clark has in common with Macdonald and Winnett that he does not refer to the basic works of both DAFA and CoDRS.

c - Winnett goes on in his demonstration saying:

"That Saf *ʾtm* had a sexual connotation, at least sometimes, is evident from C 285[2] where it is used in association with the verbs *nyk* (cp. Ar *nāka*, "had sexual relations with") and *qd* (cp. Ar *qāda*, "acted as pimp for"). *fʾtm zf* is probably to be translated "and he ripped [the genitalia] of Zawf."

Note 2 reproduces CIS's reading and translation which read as follows (p. 45B):

285. — Dunand, 1338.

לבת וניך בגרמה פאתם זף פקד אבץ

Baththo. Et futuit Bigarmihius, et rem habuit cum Zawfo. Et lenocinium exercuit 'Abiṣ.

Winnett's etymology of Ar qâdâ (o) is copied from his most favored dictionary, SaAAL (p. 823B). But, that meaning is the third in SaAAL's list and, more importantly, it is clearly introduced by "[ʾAla]," which means that that meaning is used when the complement of the verb is governed by the preposition ʿalá. Thus, Winnett cannot mention "with" because the preposition ʿl is inexistent in the Saf text. Furthermore, his translation of fʾtm zf is unacceptable.

a - As CIS, Winnett considers zf as a feminine name on the sole basis of his interpretation of ʾtm. No reason can justify such an opinion; zf is a masculine name in Ar (cf., e.g., W. Caskel, Ǧamharat an-Nasab. Das genealogische Werk des Hišām ibn Muḥammad al-Kalbī, Leiden, 1966, I, p. 605B).

b - Winnett ignores the difference between the conjunctions w and f. The first normally introduces a phrase having as subject the author of the text, and the second is normally followed by a phrase having as subject a person different from the author of the text (e.g., it introduces quite often an invocation to a deity) or by a simple word. Here follow a few examples: JaS 54a: pedigree wwld fhlt -; JaS 63: pedigree wbny fhls; and JaS 101a: pedigree wdbḥ fdšr -; etc. The preceding rule is by no means absolutely constant; exceptions are not missing: JaS 104: pedigree wngʿ fʾmḥl (f instead of w); JaS 127: pedigree wʿrf - - fndm (f replaces w and introduces a consequence of ʿrf sfr mentioned just before); JaS 153: pedigree fḥll (f replaces w); etc.

It is precisely because of the normal use of both w and f described above not only that JaSISA noted (p. 200) that "zf is the subject of the verb ʾtm," a remark which Winnett of course disregards, but also that ʾbṣ is the subject of qd, as already understood by CIS. Summarily, the result of Winnett's whole exposé is negative. (1) Although warned by JaSISA, he blindly followed into the footsteps of both ClAS and Macdonald and missed his essential goal, viz. the basic meaning of Ar ʾatama. (2) Although giving the reader the impression that he innovates in translating ʾtm as "to rip," he in fact copied ClAS's second analogical application which is none other than the translation of LiSI himself, an author who is not even mentioned in Winnett's argumentation. (3) He also copied CIS's wrong interpretation of the subject of ʾtm in CIS 285, although the error had already been denounced.

2 - Winnett devotes his second and last paragraph mainly to JaS 2, 10, 14, 17 and 19: *wʾtm ʿl hr* *And he came together with Hayr*; and JaS 3: *wʾtm hr* *And he engaged in sexual relations with Hayr* (cf. JaSISA, pp.200 foll.)

a - Winnett claims that " A comparison of
the five texts mentioned with JaS 3 leaves the reader with the impression that all six are identical in meaning " (p. 239A-B). Whatever "impression" or preconception Winnett has now or might have in the future does not alter the fact that the contents of the pericope of JaS 3 are not the same as those of the five other texts, viz. his statement is nothing more than the formulation of his thesis which is still to be proven.

b - Winnett sets out to explain his so-called absence of *ʿl* in JaS 3: he has recourse, here again, to one of his favorite ploys, viz." *the omission of ʿl in JaS 3 is probably the result of an oversight on the part of the writer.* " (p. 239B). As I stated in JaIPSC (p. 487), "what a pity that the Saf authors did not learn their native language from the 2 editors...!," the first of whom is Winnett. The 10th and last example of his "massive and unusually diversified use of voluntary irregularities" encountered by JaIPSC (p. 487) in Winnett's large collection of Saf texts, WiHaI, is stated as follows:

" *j* - pitiless buldoring of different texts even known by photographs to suit their obsession of pedigrees which, combined with a seemingly unshakable self-assurance in the matter, are responsible for their accusing Saf authors of engraving errors ."

In fact, Winnett's accusing the writer of "an oversight" is based exclusively on his preconcieved idea, which he calls his "impression," that the same formula must be found in all six texts; viz. the preposition *ʿl* annoys him and he, in turn, declares it to be an engraving error.

c - The third section of Winnett's argumentation completely deviates from the position of his first paragraph and ends up in contradicting himself by accepting another translation for *ʾtm*.

(1) He now refers to WiHaI's commentary on WH 374 which he repeats here (p. 239B) to the effect that Ar

" *maʾtam* meaning "obsequies, funeral ceremony," is evidence that a verb *ʾatama* meaning "to mourn, lament," once existed in Ar. " (p. 239B).

(a) Even at first sight, Winnett has abandoned the main concern of his first paragraph, "the basic meaning of *ʾtm*." Now, he speaks of "A verb *ʾatama*"(the capitalization is mine). It is precisely the

word *maʾtam* that is given by both DAPA and CoLHS to justify the basic meaning of the verb as implying a reunion or a gathering. That such a meaning became to be applied to "funeral gathering" and, then, to the most predominant part of funerals, viz. "tears, mourning," is quite understandable. But, to find in the word *maʾtam* the "evidence that a verb *ʾatama* meaning 'to mourn, lament,' once existed in Ar" is working backward illogically and presumptuously.

(b) In support of his point of view, Winnett refers to W.W. Müller. The latter's testimony is valueless; JaNAR, XIII, pp. 145-175, shows that "to crib and even summarize commonplaces on a selected subject does not prove expertise. But, to repeat evident errors proves that the so-called self-proclaimed expert is actually fooling nobody but himself" (p. 144).

Müller's position on *ʾtm* is dealt with on pp. 151-152.

(c) Winnett also fails to inform his reader that the very same *ʾtm* of WH 374, 375 and 377, and *ʾtm ʿl* of WH 376 is rendered in his edition as "the funeral ceremony for" (pp. 103-104) and "he lamented for" (p. 104).

[1] The reader may compare the two following statements of WiHaI, viz.

"The noun, found in 374, 375, 377, and in the plur. form *ʾtmn* in 496" (p.104);
and
"*ʾtm* (a) 'funeral ceremony' (?) 374, 375, 377, 496" (p. 628 in the lexicon).

For WH 496, where the edition does not read "*ʾtmn*," cf. below. — The question mark of the lexicon is not to be found in the editor's translation of any of the four listed texts. — The verb *ʾtm* of WH 376 is not listed in the lexicon (p.628). — The "(b)" of Winnett's quotation refers to WH 3022 which was reedited in JaMAR, XIII, p. 227.

[2] Here again, Winnett does not explain the reason why he adopted two different meanings in his edition of the texts. His initiative is all the more incomprehensible in that either meaning could be applied in each case, viz. "the funeral ceremony" and "he held the funeral ceremony for;" OR "the laments for" and "he lamented for."

[3] It should be repeated here again that Winnett's two meanings are, respectively, the first and the second derivations of the basic meaning of "gathering."

[4] Winnett fails to inform his reader that the pericope of WH 375 and 376, which were written by the same person, are not identical. Here, are the relevants parts of WiHaI's edition

(p. 104, and facsimiles on p. 668):

375 lrjl bn Šhm wbh' b'd 'tm 'ys
"By RiǰEl b. Šahm. He moved camp after the funeral ceremony for 'IyEs."

376 is by the same person.

376 lrjl bn Šhm w'tm 'l 'ys fr'y (p412)
"By RiǰEl b. Šahm. He lamented for 'IyEs and shepherded."

For the verb bh', cf. JaSN, pp. 32-33, where it is translated as to become familiar with on the basis of the same meaning being the first given by PrLAL, I, p. 163B) as well as by DAFA (II [1970], p. 861A: "to get to know s.o, to get used to sth") and CoDRS (fasc. 2 [1976], p. 47B: "comprendre, s'habituer à qe"). — There is no way of reconciling together the events mentioned by the two texts if understood as in Winnett's translation.

The two texts cannot be perfectly contemporary because the author cannot state that "he moved camp" and "shepherded" at the same place at the same time. On the one hand, if WH 375 is anterior to WH 376, how could the author of the text "shepherd" at that very spot (WH 376) since he declared in WH 375 that "he moved camp after the funeral ceremony"? On the other hand, if WH 376 is anterior to WH 375, how could the author of the texts state that "he moved camp after the funeral ceremony" since he stated in WH 376 that "he lamented for ... and shepherded" at that spot? My translation of the two texts, however, makes the chronology of the texts very clear:

WH 376: and he came together with 'Iyâs [here] and he pastured [here];
WH 375: and he became familiar [with the spot] after having sex with 'Iyâs.

The initial name of WH 496 is bly Balay (cf. Ar balîy "tritus" in PrLAL, I, p. 158A) instead of "bny" of the edition: the stroke of l is twice as long as that of n of bn, almost as long as the following y, and longer than b. HaAIC does not utilize the text. It may well be that the second half of the original text reads, as in the edition, "whh' b'd 'tm 'ys" (a formula found in WH 374, 375 and 377) but that will always remain unknown until a good photograph is released. The published facsimile, however, reads wbh' bn l'tmn 'ys And Baha', son of 'Ilatmân, was in despair. - bh', e.g., CIS 3817. - l'tmn: the tiny dot on the right of the staff of l cannot be compared with the appendix of the two y's, and has no more value than the short aslant stroke above the letter. Cf. l'tm in CIS 2172; for the nominal formation, cf., e.g., l'lmn in WH 2680. Note that WiHaI

remarks on this name (p. 393, and facsimile on p. 707) that "possibly the first stroke should be ignored," his reason being that 'lmn does exist. Winnett does not have any photograph of the text. The true reason for Winnett's suggestion is that he does not know what to do with such a name and, therefore, he suggests correcting it to something else that he already knows. That he does not know what to do with the stroke is exemplified by his lexicon where both readings, "l'lmn" and "'lmn," are listed on pp. 608A and 596B; in each case the reference is made to the other reading as a possibility. - 'ys, cf. Ar 'ayisa "to despair" (cf. LaAEL, p. 157A; cf. also DAFA, p. 326A).

Winnett's interpretation of the last section of LP 720 as "the year of the mourning for 'Aḥlam" (p. 239B) is unacceptable. The whole pericope of LP 720 is read by LiSI (p. 184) as follows: " wšyr mn mhlk snt 'tm 'hlm And he came from perilous places, in the year in which 'Aḥlam was ripped"." But, nothing in the text demands the subject of 'tm to be different from that of šyr, viz. the author of the inscription. Thus, the pericope may be translated as follows: And he journeyed from places of death the year he engaged in sexual relations with 'Aḥlam. Cf., e.g., as parallels given by LiSI himself:

LP 331: "snt r'y ḥḍ'n in the year in which he tended the sheep" (p. 83);

LP 350: "snt ḥg bt b'lsmn in the year in which he betook himself to the house of Ba'al-Samîn' (p. 90);

LP 424: "snt ngy mn hsltn in the year in which he escaped from the government" (p. 114; cf. also a similar pericope in LP 540);

LP 1190: " snt ḥrd 'l srṭt, which JaMAR, XIII, translates as follows: "in the year in which he went down to Sartat" (p. 196; srṭt is a personal name); etc.

LiSI, however, introduces exceptions to his own understanding because of some unknown reasons which I remain unable to figure out; cf.

LP 297: LiSI (p. 72) reads the last pericope as follows: "snt qtl m'n in the year in which Ma'an was killed." Here again, there is no reason why the subject of qtl should not be the author of the text. The pericope may be translated as follows: in the year he killed Ma'an. For my interpretation of the pericope immediately preceding the above quotation, cf. JaGUR, p. 29.

LP 722: LiSI (p. 185) reads the central pericope as follows: "w'lf hm'ay snt b's whgz And he fed the goats in the year of misfortune and detention."

There is no reason not only for considering both b's and hgz as nouns instead of verbs but also for not considering the subject of 'lf, the author of the text, as

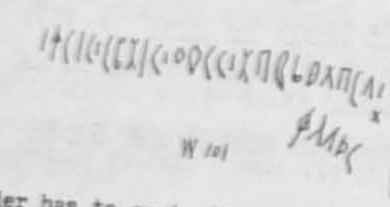
the subject of these two verbs. The text may be translated as follows: And he fed the [= his] goats with fodder [here] in the year he was in a state of distress and was hampered. — ḥjz, cf. Ar ḥajaza "to prevent, hinder, impede, withhold, restrain, debar" (cf. LaAEL, p. 519A; cf. also DAFA, III [1976], p. 2149A: A Notion de base: obstacle, barrage").

The basic remark made on LP 720 and 297 may be applied to several other texts the suggested translation of which must be revised. Here follow four examples.

CIS 16: CIS (p. 5B) reads the pericope of the text as follows: "snt qtl ʿdrʾln scil. no quo interfectus est ʿAḏharʾilân." For the central section of the text, cf. JaSN, p. 49, note 41. — The dot below "d" of the translation is of course a printing slip. — The text may be translated as follows: the year he killed ʿAḏarʾilân.

CIS 3680: CIS (p. 466A) reads the pericope of the text as follows: "wġzm snt ḥrb nbṭ Et praedatus est anno belli Nabaṭ." The pericope may be translated as follows: And he made a raid the year he plundered the Nabataeans.

Wi 851: WiSIJ (p. 176) reads the text as follows:



851. (W 101) lqbl bn (ġ)tʾl bn ʿgb bn ʾġmy wtġr snt ghr db

By Qabil b. (Gh)autḥʾil b. ʿAgib b. ʾAġmay. And he was on the look-out the year of the conquest of DB.

GHR - cf. ġahr, "ill-treatment, oppression, conquest."

The reader has to go to the "Word List" to find out that "db" is understood as a "n. loc." (p. 159). No reason is given to prove that the word could not be a personal name since Ar ġahara means "to overcome" (cf. LaAEL, p. 2569C). Furthermore, the same list gives "qh(r)" (cf. also p. 170) instead of "qhr" of the edition. Yet, the facsimile clearly shows s instead of either "r" or "R." However, one of the three letters after snt was miscopied because the pericope as it stands is untranslatable. Both the first and the third letter forms are confirmed by the second and the 26th letters, this letter being s of snt. But, the second letter seems abnormal if compared with the ninth and the 18th letters, two ʾ's, viz. the section of the letter formed by the acute angle takes more than 1/2 of the total height of the letter, 4.5 and 7.5 mm, respectively, while the same section of the ʾ's is at the most 3.2 out of 8.2 mm. I suggest that the second letter is a miscopied s. Finally, the initial letter of the second name of the pedigree is read "(ġ)" in the transliteration but "(Gh)" in the translation. The text is not listed under "ġtʾl" (p. 185); and there is no "gtʾl" (p. 151). HaAIC does not list

the text under "ġtʾl" (p. 452) and utilizes only two out of the five names of the text, viz. ʿgb (p. 407) and db (p. 235, where it is considered as a possible place name). The facsimile makes it clear that the letter is r; thus, rtʾl. — The text may be read as follows: lqbl bn rtʾl bn ʿgb bn ʾġmy wtġr snt q(s)s db By Qâbil, son of Rattʾil, son of ʿÂgib, son of ʾAġmay. And he was on the lookout the year he was pursuing Dabb. — q(s)s, cf. Ar qassa "to seek after, pursue" (cf. LaAEL, p. 2521A).

WiHaI (p. 325) reads the second half of the text as follows:

wwġzz snt ḫrb nbṭ fhyṭʿ flṭ (p449)

He fought the year of the Nabataean war.

O Yitaʿ, deliver [him]."

Here again, there is no reason for not maintaining the subject of ġzz, viz. the author of the text, as that of ḫrb. Furthermore, Winnett gratuitously invents an inexistent personal pronoun at the end of the text to render his translation intelligible; flṭ here is a noun, e.g., as in LP 495 and 504 for which cf. JaMAR, XIV, pp. 159-160 and 157, respectively. - The pericope may be translated as follows: And he fought the year he plundered the Nabataeans. And, O Yitaʿ, [bring] liberation.

(2) Winnett then applies his second interpretation of ʾtm "to mourn, lament" to JaS 3, etc: " Thus ʾtm (?) ḥr probably means "he mourned for Ḥayr." " (p. 239B).

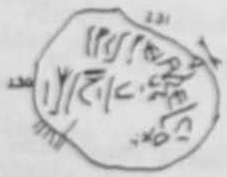
Winnett commits here the grave error of accepting as a self-evident fact, which he does not even bring to his reader's attention, that a verb expressing feelings, such as would be his "ʾtm," would govern its complement without any preposition. It is a well-known fact that all those verbs expressing feelings have a preposition to govern their complement(s) (cf. above, pp. 99-102) in Saf and in Ar as well.

3 - The preceding study of Winnett's theory would remain incomplete without the following paragraphs which show how illogical Winnett can be, viz. he makes his own rules; builds up his own argumentation upon fleeting whims and, in order to protect his little game, he simply hides from his reader the embarassing texts, many of which he himself published.

a - The part most obviously missing in his Saf section is his study of both van den Branden's and Clark's opinions on Saf ʾtm. His unique reference to ClAS's research (cf. above, p. 128) was a mere window dressing since he quotes a statement which is not a part of, and does not have any effect on,

the author's opinion. However, by a strangely logical organization of his notes, it is toward the end of his introduction to the T h a m texts that Winnett informs his reader of CIAS's opinion which deals exclusively with S a f (cf. below, C 5). But, van den Branden's theory will never be given ... even a mere reference!

b - The underlying motivation of Winnett's switch in his approach to ʾtm is that none of the Saf texts shows any sexual overtone and, therefore, ʾtm's meaning of these texts must, in his opinion, be different from that of CIS 285 and should better be expressed as "to mourn, lament." Winnett is not even aware of his illogical attitude. One example suffices here. If the verb nyk obviously belongs to the sexual vocabulary, what about the verb nk? Were Winnett logical with his basic stand on ʾtm, he should have avoided like the bubonic plague equating nk with nyk in the texts which do not otherwise contain any clue or overtone of sexuality. Such is the case of both WH 231 and 520, both published by WiHaI, which do NOT contain THE SLIGHTEST detail beside the verb nk: WH 231 (p. 80, and facsimile on p. 664): the pedigree + dnk šḥl bn ʿt." and WH 520 (p. 124, and facsimile on p. 671): the pedigree + nk. There is absolutely nothing in those texts which might even remotedly remind the reader of anything sexual. Yet, Winnett equates nk with nyk in each case and interprets it as "'to have sexual relation with' (Ar. nāka)" (p. 649). Were he logical with his stand on ʾtm, he should, and would, have discarded the nyk parallel in favor, e.g., of nkk; cf. Ar nakka "to urge, press upon, beset."



WH 231: Winnett's reading must be revised (p. 80, and facsimile on p. 664):

231 lybġy ḏnk šḥl bn ʿt. (p96)
"By YBĠY who had intercourse with ŠḤL b. 'T.."

He reads the third letter as "b" without any comment and the name as "ybġy." The letter is r, composed of two straight strokes making a very wide angle, as opposed to the well-rounded curve making a narrow loop in b. He eliminates a letter after k; reads "ʿ" instead of m; and does not read the last letter, "ʿt.," which may be an incomplete k, the two dots representing the tail ends of the two short strokes; that letter is also eliminated by HaAIC, p. 404: "ʿt." The text may be read as follows: lyrġy ḏnkl šḥl bn mt(k) By Yarġay, who has punished Šaḥil, son of Mât(ik). — yrġy, cf. rġy of CIS 2164. — The use of the relative pronoun is unusual in such a context. - nkl, cf. Ar nakala, 2nd form with or without the presposition bi, "punir" (cf. DoSDA, II

[1967], p. 723B). — mt(k), cf., e.g., mtwk in LP 1208.

In WH 2146 (cf. WiHaI, p. 331, and facsimile on p. 699), the second name is hmʾns Hamuʾannis (cf. Ar ʾanasa, 4th form "to behave in a sociable manner;" cf. LaAEL, p. 113B) instead of the edition "ʾmʾns" (also p. 554B). The letter h is very clear in the facsimile, and Winnett does not even put his "ʾ" between parentheses.

2146 lbqqn bn 'm'ns
"By BQQN b. 'M'NS."

c - Although he has reached the end of his Saf section, Winnett has failed to explain an earlier statement, viz. "That Saf ʾtm has a sexual connotation, at least sometimes" (cf. above, p. 128, for the full text). So far, he gave only one example, CIS 285. As "sometimes" means "on some occasions, occasionally," the reader cannot but be disappointed at not being informed anything about the other examples which Winnett must have had in mind. It is also almost incredible to see Winnett referring only to WH 374 in his so-called study. Whether he consulted his cards or WiHaI's lexicon (p. 628), he could not help seeing that ʾtm is also listed in WH 3032.

3032 lṣ'd bn 'ys bn ḫr (w)rḥṣ wb'lsmn 'tm
"By ṢA'id b. 'IyAs b. ḪAir. He sweated and Ba'lsamîn restored [him] to health."

'tm is probably the IVth form of tamma, "to make complete, whole." It occurs frequently in Tham. with the meaning "to restore to health."

Why does he sweep that text under the rug? Was it because JaMAR, XIII, p. 227, not only pointed out his misreading of four letters, his eliminating of another, and his inventing of an inexistent personal pronoun to give some meaning to his translation, but objected also to him that "the sweat is common to all persons and is not a disease? The translation of the god's act is, thus, uncalled for". What is important is Winnett's reading of the text, which his recent paper does not reject; that he jumped into a third swing and translates Saf ʾtm as "he restored to health"(also p. 628). What would be so unusual in a phrase (evidently in the line of his theory on ʾtm) such as this: "He sweated and Baʿlsamîn! - he mourned"? Winnett's reason for his new switch is the presence of the divine name bʿlsmn which, in his opinion, must be followed by a verb with the divine name as its subject. Such a premise is a fallacy (cf. more expli-

the author's opinion. However, by a strangely logical organization of his notes, it is toward the end of his introduction to the T h a m texts that Winnett informs his reader of CIAS's opinion which deals exclusively with S a f (cf. below, C 5). But, van den Branden's theory will never be given ... even a mere reference!

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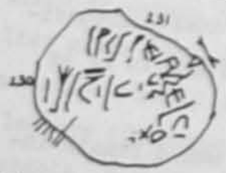


231 *lybġy dnk šḥl bn ʿt.* (p96)
"By YBĠY who had intercourse with ŠḤL b. 'T.."

He reads the third letter as "b" without any comment and the name as "*ybġy*." The letter is *r*, composed of two straight strokes making a very wide angle, as opposed to the well-rounded curve making a narrow loop in *b*. He eliminates a letter after *k*; reads "ʿ" instead of *m*; and does not read the last letter, "ʿt.," which may be an incomplete *k*, the two dots representing the tail ends of the two short strokes; that letter is also eliminated by HaAIC, p. 404: "ʿt." The text may be read as follows: *lyrġy dnkl šḥl bn mt(k) By Yarġay, who has punished Šaḥil, son of Mât(ik).* — *yrġy*, cf. *rġy* of CIS 2164. — The use of the relative pronoun is unusual in such a context. - *nkl*, cf. Ar *nakala*, 2nd form with or without the presposition *bi*, "punir" (cf. DoSDA, II

[1967], p. 723B). — *mt(k)*, cf., e.g., *mtwk* in LP 1208.
In WH 2146 (cf. WiHaI, p. 331, and facsimile on p. 699), the second name is *hmʾns* *Hamuʾannis* (cf. Ar *ʾanasa*, 4th form "to behave in a sociable manner;" cf. LaAEL, p. 113B) instead of the edition "*ʾmʾns*" (also p. 554B). The letter *h* is very clear in the facsimile, and Winnett does not even put his "ʾ" between parentheses.

2146 *lbqqn bn 'm'ns*
"By BQQN b. 'M'NS."

c - Although he has reached the end of his Saf section, Winnett has failed to explain an earlier statement, viz. "That Saf *ʾtm* has a sexual connotation, at least sometimes" (cf. above, p. 128, for the full text). So far, he gave only one example, CIS 285. As "sometimes" means "on some occasions, occasionally," the reader cannot but be disappointed at not being informed anything about the other examples which Winnett must have had in mind. It is also almost incredible to see Winnett referring only to WH 374 in his so-called study. Whether he consulted his cards or WiHaI's lexicon (p. 628), he could not help seeing that *ʾtm* is also listed in WH 3032.

3032 *lṣʿd bn 'ys bn ḫr (w)rḥḍ wbʿlsmn 'tm*
"By ṢʿID b. 'IYĀS b. ḪAIR. He sweated and Baʿlsamīn restored [him] to health."

'tm is probably the IVth form of *tamma*, "to make complete, whole." It occurs frequently in Tham. with the meaning "to restore to health."

Why does he sweep that text under the rug? Was it because JaMAR, XIII, p. 227, not only pointed out his misreading of four letters, his eliminating of another, and his inventing of an inexistent personal pronoun to give some meaning to his translation, but objected also to him that "the sweat is common to all persons and is not a disease? The translation of the god's act is, thus, uncalled for". What is important is Winnett's reading of the text, which his recent paper does not reject; that he jumped into a third swing and translates Saf *ʾtm* as "he restored to health"(also p. 628). What would be so unusual in a phrase (evidently in the line of his theory on *ʾtm*) such as this: "He sweated and Baʿlsamīn! - he mourned"? Winnett's reason for his new switch is the presence of the divine name *bʿlsmn* which, in his opinion, must be followed by a verb with the divine name as its subject. Such a premise is a fallacy (cf. more expli-

citely below, D 4). Here follow a few examples.

The first example is WH 2471 published by Winnett himself in WiHaI (p. 369, and facsimile on p. 704). He reads the text as follows:

2471 hb'lsmn blḥ (')ḫ (p545)
"O Ba'lsamīn, (')ḫ is weary."

Winnett's reading of his "(ʾ)" is yet another example of his irrational approach to the texts. The facsimile clearly shows h, and he has a photograph of the text. Yet, in spite of it, he corrects h to "(ʾ)." His reasoning is quite obvious: ʾḫ is well-known, but hḫ is new. For the name hḫ, cf. Ar hīḫ "vox, qua demittere se in genua iubetur camelus" and hīyaḫ "camelus mugiens, cum procumbere iubetur voce hīḫi" (cf. FrLAL, IV, p. 423A).

Other examples are as follows:

JaS 88: wlt sgnt And, Lat, imprisonment;
JaS 140b: wlt wdš bḫwln And, Lat, [there were] disorders in Ḫawlān;
JaS 159b: fhlt wdš fslm And, O Lat, [there were] disorders [here], and he was safe."

The above-mentioned pericopes are exclusively informative of facts important to the different authors; they were not at all imprecative: the deity is neither asked to grant any favor nor held responsible for imprisonment or disorders.

The preceding paragraphs have shown that Winnett sweeps under the rug some texts, a few of which he himself published, which are embarassing - to say the least - for his thesis.

d - Note on bʿlsmn. - WiHaI (p. 559B) lists WH 2411 under the divine name. But, in his edition of the text, he reads "bʿl smy" (p. 361) and the final y is on the facsimile (p. 703). His reading of "bʿlsmn" must have been his initial decipherment of the text which manifests his desire of adopting what he thought the text should have. Later on, he revised his decipherment and interpretation as "bʿl smy he married SMY" without correcting his initial name lists (The evidence of two stages in WiHaI was already pointed out by JaIFSC, pp. 481-483). HaAIC does not utilize this inscription. But, he seems to endorse Winnett's reading of "brkt" because he lists the name as being 16 times as a place name in the WH collection, and this is the number of references in WiHaI (p. 558B); he does not endorse "nʿm" as a place name as in WiHaI because he does not list "nʿm" as a place name; neither does he accept Winnett's interpretation of "smy" as a personal name because he does not list it on p. 331.

2411 lnǧbr bn 'ṣlḥ wqrr brkt w'ḫsf n'm snt b'l smy (p516)
"By Naǧbar b. 'Aṣlaḥ. He settled at BRKT and the soft tract of N'M the year he married SMY."

The basic meaning of Ar baʿl is "lord, master, possessor" (cf. DAFA, I [1967], p. 722B; cf. also C.-F. Jean — J. Hoftijzer, Dictionnaire des inscriptions sémitiques de l'Ouest, Leiden, 1965, p. 40). — Ar qarra "to settle, rest, remain, reside" governs its complement with the preposition bi or fī (cf. LaAEL, p. 2499A and FrLAL, III, p. 415A); cf., e.g., dṯʾ b- to spend the spring at (e.g., JaS 127) and šṯw b- to spend the winter at" (e.g., JaS 175). The preposition b is the 15th letter, but Winnett erroneously takes it as the initial letter of the following place name. — Winnett does not refer qrr to CIS 2458 by ignoring JaSN, p. 52, note 47. — The third letter of his "brkt" cannot be "k;" it looks like an incomplete ḫ. — "ʾḫsf" is considered by Winnett as the plural of "ḫsf," but he translates it as a singular. His interpretation of the word is unacceptable because the preposition b should have been repeated before his "ʾḫsf." — The sixth letter from the end is read as "b" of his "bʿl;" but there is still a tiny tail visible on his facsimile. — The long pericope of the text may be read as follows:

wqrr br(ḫ)t wʾḫs fnʿm snt sʿl smy
And he remained in Ra(ḫḫ)at and found Faniʿum to be contemptible the year Samay coughed.

r(ḫ)t, cf., e.g., the personal name rḫ in Wi 226. — The two verbs qrr and ʾḫs are each introduced by the presposition w and, thus, have the author of the text as subject (cf. above, p. 129). — ʾḫs, cf. Ar ḫassa, 4th form "to find someone to be contemptible" (cf. LaAEL, p. 736A). — fnʿm, CIS 1119. — sʿl, cf. Ar saʿala "to cough" (cf. l.c., p. 1365B).

In order to facilitate the reading of the following pages, it seems preferable to repeat here the contents of the texts involved:

JaS 2: lklb bn ntn bn frḫ wʾtm ʿl ḫr ḏʾl bhʾ;
JaS 3: lmḥsm bn šdt bn mḥlm wʾtm ḫr ḏʾl ḫlfn;
JaS 10: lʿḏrʾl bn ḥnn bn ʿḏrʾl wʾtm ʿl ḫr wtly ʿl ḫr ṯyʾ fḫrb;
JaS 14: lġṯfn bn srdt bn ġṯfn wʾtm ʿl ḫr;
JaS 17: lmṯ bn rbḥt wʾtm ʿl ḫr; and
JaS 19: lwʾlt bn snm wʾtm ʿl ḫr.

JaSISA translates *ʾtm hr* of JaS 3 as *He engaged in sexual relation with Hayr* (p. 200), but *ʾtm ʿl hr* of the five other texts as *He came together with Hayr*" pp. 198, 205, 208, 210 and 211, respectively).

4 - A. van den Branden's opinion (cf. al-Machriq, 63 [1969], pp. 733-744).

The author's basic consideration, which determines the field of his thinking, is his etymology of *hr* from Ar *harr* "fente; mort;"then "tombeau" because "les cairns auxquels ces pierres inscrites ont appartenu sont, en effet, des tombeaux" (p. 736).

Firstly, it is irrational to seek the meaning of a verb in that of its complement. Secondly, these stones originate from the country of ʿArʿar from where the inscriptions of JaSICA come from. Many of them were not found on cairns, and it is impossible to prove that those stones were originally parts of those cairns. As stated by JaSICA (p. 42): "these cairns or heaps of stones are landmarks or rally signs." I do not see what would be gained here by taking up every major argument developed by the author. Therefore, I shall limit myself to his remarks on my understanding of *ʾtm/ʾtm ʿl* for the main purpose of pinpointing the origin of some of Clark's unacknowledged borrowings. — van den Branden's remarks read as follows (p. 735):

a..... On ne s'attend pas à une pareille inscription sur un
b..... tombeau. D'ailleurs, les textes 3, 4 et 5 dans lesquels figure cette
c..... formule, forment un tout. Ce sont des textes tracés par un père
d..... en présence de son fils et de son petit-fils. Paléographiquement ils
e..... s'avèrent être écrits par la même main. D'après la traduction du
f..... P. Jamme, il s'agirait d'une pratique d'homophylie. Certes, l'homosexualité n'était pas considérée par les anciens Arabes comme une pratique particulièrement honteuse, mais il nous semble peu probable qu'un père de famille s'en vanterait en présence de son fils et de son petit-fils.
g..... D'autre part, il n'est pas sûr que la préposition *ʿl* ait manqué à cette expression. Elle nous semble avoir disparu par suite de l'éraflure qui couvre cette partie de l'inscription. Il y a, en effet, à en juger d'après la photographie publiée, assez d'espace pour la restitution de deux lettres, le *ʿ* et le *l*, deux signes qui ne demandent que très peu d'espace. Et même supposé qu'elle manque, la signification de l'expression ne différerait pas nécessairement de celle qui contient la préposition comme le montre p. ex. le verbe *ugm ʿl* qui se présente également sans la préposition, sans que cela affecte son sens (1).
h.....
i.....

(1) Cf. LittmannSyr. ad *ugm*.

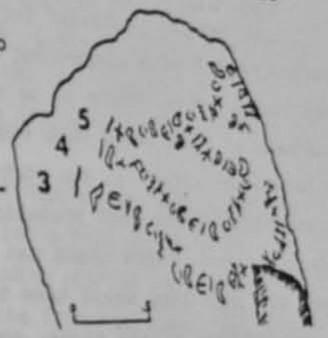
a is valueless since there was no tomb in the vicinity of the place where those stones were found.

b is nothing new: it is most obvious to anyone who looks at the facsimiles and at

JaSISA's genealogical tree of the three authors.

It is grossly erroneous not only because each text was written by the person who says to have done it but also because of JaSISA's genealogical tree and commentary which clearly states that "the authors of JaS 3 and 4 are brothers and nephews of the author of JaS 5" (p. 203).

van den Branden never refers to the preceding twofold information, and his statement is based on his misreading of the name of the author of JaS 3, viz. his "*mhlm*" instead of *mḥsm*: the letter *s* is clear on the published photograph as it is on my tracing:



Here follows JaSISA's genealogical tree dissected into its three elements with van den Branden's erroneous pedigree based on his misreading of a single letter:

JaSISA			van den Branden		
mhlm	mhlm		mhlm	mhlm	mhlm
šdt	šdt		šdt	šdt	šdt
mhlm	mhlm	mhlm	His mhlm	mhlm	mhlm
šdt	šdt	tm		šdt	tm
mḥsm	msk			msk	
JaS 3	JaS 4	JaS 5	JaS 3	JaS 4	JaS 5

Of course, it crossed my mind that *šdt bn mhlm* of JaS 3 could be the same as their homonyms at the top of the pedigrees of JaS 4 and 5. I never thought that such an opinion was probable. Indeed, *mḥsm* of JaS 3 would then be the uncle of *tm* of JaS 5 and the granduncle of *msk* of JaS 4 who would become a nephew of *tm* of JaS 5. It seems much easier for two brothers and their uncle to be together and use the same stone to engrave their pedigrees than for three relatives belonging to three different generations and three different branches of a family to be together and use the same stone for their pedigrees. According to JaSISA's genealogical tree, the order of the texts is logical: the upper text is by the uncle and the two below by his nephews. Otherwise, the upper text would be by a man, that below it by his nephew and the bottom text by the first man's uncle.

d is erroneous: the initial *m* is different in each text; so is the form of *d*, *h* and *s*.

e also is erroneous: the homophylie has nothing to do with any aspect of sexuality as it refers to biological characteristics of animals.

f is a personal judgment on the part of van den Branden, and that kind of statement is inadmissible. Not a single text published so far even remotedly indicates that homoseruality was considered as "honteuse" by those populations. It is appropriate to recall here my remarks on a similar case, viz. G. Ryckmans' evaluation of some petroglyths; they read as follows:

> "Ces trois dessins ne sont ni plus ni moins déconcertants ni plus déshonorants que certains dessins thamoudéens auxquels l'éditeur [= G. Ryckmans] ne fait pas allusion, p.ex. Philby 345b; ils relèvent tous d'un réalisme naturaliste qui doit être compris uniquement en fonction des civilisations qui les ont produits" (cf. BiOr, 17 [1960], p. 262A).

g is the result of a misreading of both the photograph and the tracing which is reproduced above. There is no room for three letters, two of them being m and ʾ and each of them being much wider than the narrow stroke of l: the letter m must have been aslant to the right for it is completely destroyed by the splinter, and this letter most probably took the whole width of the upper section of the damaged spot. There could not possibly have been two letters added there: had it been the case they would have been engraved, at least partially, on the still undamaged surface of the stone, and nothing is visible there.

h: the use of prepositions after the verbs normally affects deeply the meaning of the latter. An example, which is perfectly parallel to the present case, was given by JaGUR (p. 29), viz. ġzz (3rd form) to fight; and

ġzz b- (1st form) to select for oneself.

It would, of course, be ridiculous to translate

wġzz bhmʿzy (CIS 4452) as "he fought in the [= his] goats" or even "he fought the [= his] goats;"

wġzz bbql (Wi 976) as "he fought in the [= his] grass" or even "he fought the [= his] grass."

It is, therefore, temerarious to start any study on the hypothesis that a particular case is an exception.

i also is erroneous. Two years before the printing of van den Branden's paper, JaSVW had stated (p. 168): "The usage of the verb wġm with ʿl is so overwhelmingly consistent that it would be temerarious still to maintain (on what basis?) the probability of ever finding wġm with a direct complement; wġm may safely be considered as a static, an intransitive verb."

5 - V.A. Clark's opinion.

The centerpiece of ClAS's study of ʾtm/ʾtm ʿl (p. 156, note 42) is an extravagant application of an expression involving the Ar verb tamma combined with the old error of disregarding the importance of the preposi-

tions. These two main errors are enhanced by brightly colored trimmings, which include the surreptitious copying of some of van den Branden's errors. For Winnett's short reference to Clark's opinion in the Tham section, cf. below, D 3.

a - The first statement of ClAS's exposé is the main blast of his barrage, which is concisely worded as follows:

> " The expression ʾtm ʿl is to be related to the Ar. tammama or ʾatamma ʿalā, "to finish off a wounded man" (see Lane, Lexicon, p.315)."

First of all, it would have been absolutely necessary for him to prove that Saf ʾtm does not, and could not, correspond to Ar ʾtm, which is the obvious etymology. Furthermore, LaAEL does not give a single example of the use of Ar ʾatamma ʿalā. Finally, ClAS's statement completely distorts LaAEL's testimony which reads as follows (p. 315B):

تمّم على الجريح ‡ *He hastened and completed the slaughter of the wounded man; or made his slaughter sure, or certain.* "	The idea of "hasted and completed the slaughter of" = "his slaughter" = ClAS's "finish off" is clearly demanded by the word jarīḥ which Clark prefers to eliminate from his quotation of the original text, first statement. Yet, it is precisely that word that is the reason for the change of meaning given to tammama. Indeed, LaAEL himself inserted in his text the siglum ‡ which, according to his p. xxixC, "means asserted to be tropical," viz. 'rhetorically changed from its exact original sense, having the nature of a trop." It must be strongly emphasized that none of the persons called hr's is mentioned in any text as being wounded; therefore, the Ar expression is not, and cannot be, applicable in the present case.

b - ClAS's second blast is presented as follows:

> " Jamme's rendering of this word is inconsistent in that in JaS 2, 10, 14, 17 and 19 he has translated ʾtm ʿl ḥr as "he came together with Ḥayr" while in JaS 3 he has read ʾtm ḥr, "he engaged in sexual relations with Ḥayr". "

Clark's judgment is apparently based on this other affirmation:

> " Even if the word ʿl is missing in this case this does not necessarily imply a different meaning for the text ,"

with reference to van den Branden; for this, cf. above, p. 142 h. Taking into consideration the contrast of the preceding statement with Clark's affirmations dealt with in the next section, in which he steals van den Branden's ideas without acknowledging his source, the reader cannot but wonder why Clark felt the necessity of openly hiding behind van den Branden in this case. Furthermore, the mere enunciation of his second remark indicates that the opposite is normal, but non cons-

tant. It is, therefore, irrational to a priori choose an exception instead of the rule for an exception to a rule has to be proven and cannot be assumed gratuitously. My translation of both expressions ʾtn and ʾtn ʿl is consistent with the absence and presence of the preposition ʿl. The sexual character of CIS 285, where ʾtn is WITHOUT any preposition, is clear. But, when the verb ʾtn is followed by that preposition ʿl, I naturally reversed to the original meaning of the verb.

c - The best assortment of highly colored trimmings is found in CLAS's following quotation:

> "it ..... [A]
> seems unlikely that six different authors would have recorded ..... [A]
> that they "came together with Hayr (sexually)", presumably at ....[B]
> the same place at the same time. JaS 3, 4 and 5 are all on ...[C]
> the same stone and are texts by a father, son and grandson!

[A] is grossly erroneous based as it is on his voluntary distortion of my translations by buldozing all of them into a single one involving sexuality although only one of them, JaS 3, has such a connotation. As repeatedly stated above, only ʾtn + direct complement (JaS 3) is understood as involving sexuality while ʾtn ʿl + complement (JaS 2, 10, 14, 17, 19) is translated as he came together with. Furthermore, it does not seem very strange that the hr after ʾtn ʿl should have had a meeting with the authors of the five texts since, as stated by JaS 10, he must have been an important person of the tribe Halfân for the author of the text was his equerry (cf. below). In order to prevent any possible confusion with his homonym, hr listed after ʾtn in JaS 2 is given his tribal affiliation which is different from that of the other hr. Needless to say that the preceding reasoning should be turned all the way around if JaS 3 was engraved after JaS 2; the temporal relationship between these two texts will always remain unknown.

[B] is an oversimplistic and irrational deduction from van den Branden's b.

[C] is the copy of van den Branden's c. Thus, in spite of the published photograph, the tracing and the study, Clark endorses van den Branden's gross misreading of mhm of JaS 3 as "mhlm."

d - The grand finale of Clark's empty barrage reads as follows: "Apparently the authors of JaS 2, 3, 10, 14, 17 and 19 were all involved with the finishing off of the wounded HR." This affirmation repeats the three majors errors of his thesis to which he adds a fourth one, viz. he buldozes both ʾtn and ʾtn ʿl into a single expression; he makes up the fact that the person hr was "wounded" in an attempt at finding in the Saf text the counterpart of ja-

rih of the Ar expressions;

he restates his application of a tropical Ar expression to an unsuitable text; and

he explicitely buldozes into a single person the two hr's of the collection.

Furthermore, Clark does not seem to be aware of the ridicule of his statement according to which a gang of no less than six persons was involved in the killing of a single wounded! The memory of a display of such fierce a bravery should of course not be lost for posterity: it was more than worthy of being commemorated in no less than six inscriptions.

e - There remain two secondary trimmings of Clark's argumentation; they corroborate the general evaluation of his work by giving the reader two more examples of his incompetence.

(1) According to CLAS, my vocalisation of the name hr as Hayr cannot be accepted:

> " (not Hayr as read by Jamme - there can be no reason to assume a diphthong -ay- in this name). "

If his remark is taken seriously, then Clark relates the name hr to a root "hr" which does not exist. But, since he does not vocalize names, the reader will never know to which root he might refer the name. In fact, his remark does not make any sense unless he has the qtyl qutayl derivation in mind, in which case he would show his ignorance because the "diphtong -ay-" belongs to the radical, not to the diminutive nominal derivation. Winnett also endorses my vocalization of the name as "Hayr" (p. 259B) in spite of his spelling of "Hair" in WiSaI(p. 571B), but makes no comment on ClAS's above-quoted statement.

(2) Clark's objection against van den Branden's interpretation of ʾtn ʿl as "s'arrêter à" is this:

> "It may mean "to remain" but not "to stop" and the preposition ʿl would be inappropriate in such a context."

The answer he should have given van den Branden is the Ar expression ʾatama bi l-makâni "he stayed,remained, dwelt, and abode in the place" (cf. LaASL, p.130) with the local preposition bi, and the preposition b does not exist in the Saf expression. The second half of Clark's statement comes from an ignorant; cf., e.g., the following expressions hrs ʿl to be on the lookout for; and

dtʿ ʿl to spend the spring at.

It is difficult to understand how and why, in order to prove that ʾtn equals ʾtn ʿl, neither Winnett nor Clark did refer ... to hrs which equals hrs ʿl. This fea-

ture was easy to find since both forms are side by side in the WH collection, viz.

WH 1902: hrṣ hʾblt mšgrt *he was on the lookout for the female camel with a small litter* in JaIFSC, p. 516; and

WH 1901: "hrṣ ʿl ʾbh *He was on the lookout for his father*" as in WiHaI (p. 297, and facsimile on p. 694). Note that the facsimile does not justify the editor's addition of "..." after ʾbh.

The example of hrṣ = hrṣ ʿl proves that the rule on the use of the prepositions is not constant, not that the rule is inexistent.

f - The five preceding sections belong to ClaAS's note 42 (p. 156) which is the author's commentary on his interpretation of JaS 10 (p. 152). JaSISA (p. 205) reads the text as follows:

> *Fgr'l bn ḥnn bn 'gr'l w'tm 'l ḥr wtly 'l ḥr ty' fḥrb*
> *By 'Aḏar'il, son of Ḥannân, son of 'Aḏar'il. And he came together with Ḥayr and he was equerry for Ḥayr, going to and fro, and he fought.*

Clark translates the second half of the text as follows (p. 153):

> " and he finished off HR, and TY' had followed after HR and waged war". "

The reading of such a pericope makes two remarks necessary. The author's desire of rendering the preposition ʿl in his translation or any other pretext cannot justify his grossly erroneous use of the preposition "after" with the transitive verb "to follow." Furthermore, his interpretation does not make any sense for, if the author of the text "finished off Hr," how could Clark say that "TY' had followed HR"? How can anyone "follow after" a dead man? If, by any chance, Clark intends to say that, although a whole tribe was pursuing hr, it was the author of the text who, along with five of his friends (according to Clark), "finished him off." This does not make any sense either.

As stated above, his interpretation of ʾtm ʿl is unacceptable not only because of his etymology but also because nothing in the texts substantiates his idea on "the wounded HR." His interpretation of the central section of the pericope is also contradicted not only by the conjunction w which normally does not introduce a change in the subject of the verb but also by the grammatical order of the words. The conjunction w introducing both verb ʾtm and tly indicates that their subject is the author of the text, and were ty' the subject of tly, the word would have been written between tly and ʿl hr.

On the etymology of tly, Clark writes: "tly - tallā, 'to follow'" (p. 156). It is incomprehensible why he chose the second form of Ar talá (o) since the meaning of the 1st form is "to follow." Furthermore, Ar talâ (o) is a transitive verb; cf. the expression "talawtuhu I followed him or it, or walked behind, or after him or it" (cf. LaAEL, p. 313B). There is no preposition in Ar, and either "behind" or "after" belong idiomatically to "walk," and not to "follow."

Clark's understanding of ty' as a clan name is simplistic; cf., e.g., bh' is known as both a tribe name (JaS 2 and 13) and as a verb (e.g., LP 1214 in JaSN, p. 33). The use of f before hrb, contrasting with that of the w's, seems to introduce a consequence of the author of the text becoming an equerry of hr. Finally, the only addition to my commentary on the text is to point out that ty' could well be an asyndetic relative phrase meaning "[who] was going to and fro."

D - The Tham section of Winnett's exposé.

1 - The number of texts involved.

Winnett lists "26" (in his summary) entries which become "some twenty inscriptions" (p. 239A). The only two entries added to my list which, as stated in JaTV (p. 291A), "does not claim to be exhaustive," are Hu 627 (p. 242) and 796 (p. 243A), respectively. In fact, Hu 627 only can be added because ʾtm, either as a noun or a verb, is inexistent in Hu 796. Both texts are studied below in D 7 1 and D 7 t, respectively. Moreover, for my exclusion of the last two entries of Winnett's paper, cf. below, D 7 a (5). Finally, Winnett excludes JsaT 754 from his list and, here again, he does not justify his attitude.

2 - The seven-legged support of Winnett's theory.

Winnett's theory as illustrated by his translations is based on seven pillars each having its own special importance. The first is all too common to his publications, cf. also above, his Saf. The third, his theory on the word divider, has no direct relevance to the interpretation of ʾtm. Winnett brings up the third question in several of his translations, and the second, although neither discussed nor even mentioned by him, is nonetheless present in his treatment of four inscriptions.

a - The repeated use of inconsistent and/or contradictory statements originating from the author's shortsighted argumenting method which is riveted to the matter immediately at hand, is a feature common to Saf and Tham sections of Winnett's paper. Here follows the example of wddh of Hu 104 where he really shows himself at his best. He writes this (pp. 240B-241A):

" Jamme draws attention to the fact that in six of the inscriptions (*Hu 104, 126, 263, 317, 749, 769*) *ʾtm* is followed immediately, with one exception, by *wdd* (cp. Ar. *wadda*, "to love"). The close association of *ʾtm* with a verb meaning "to love" confirms him in his opinion that *ʾtm* had a sexual connotation.

The word *wdd*, which he takes as a verb, is almost certainly a noun (cp. Ar. *wadd, widd, wudd*, "lover, friend"). See no. 4, 8, 9 and 15 below. "

The following remarks are in order. (1) His expression "almost certainly" is given two different meanings, viz. either "with the definite exclusion" with regards to my interpretation of wdd as a verb which is squarely shelved, or "more probably" with regards to his own interpretation for his translation of wdd as "friend" in three out of the four texts listed by himself, Hu 126, 317 and 769, is followed immediately by an alternative put between parantheses, viz. "(or WDD)," a personal name.

(2) Winnett rejects Clark's opinion because " the contexts in which this expression occurs in Saf do not support his interpretation. " (p. 240A; cf. below, D 3). In the present case, the texts provide a sexual context proven by the word wdd ... and he himself refuses to acknowledge those contexts ... and his magical reason is his text reproduced above "The word wdd..." with reference to four texts only, Hu 126, 263, 267 and 769!

(3) Winnett knows very well that the words "love, lover; to love..." always have a sexual meaning. Therefore, his preconcieved theory left him no choice but to attempt to bastardize the wdd confirmation of my sexual interpretation of ʾtm; and his twofold attempt is presented in the two following paragraphs.

(4) Winnett is so blinded by his theory which he must "par belle ou par laide" implement that he does not even see how ridiculous it is, in order to avoid the idea of "love," to use the vain subterfuge of the distinction between the substantive "love, ..." and the verb "to love."

(5) WiHaI refers wdt (WH 2194) only to "Ar. wadd, 'love'" (pp. 338 and 650). For this text, cf. below, D 7 b (2) [a] [5]. WiReAES's p. 93B contains this: " *wd*
wdd
n. "lover" 71
v. perf. "to love" (*wadda*) 19, 20, 37, 43a, b, 45, 46, 61, 76, 89, 114, 128, 156, 166, 188÷187, 196, 203d
n. "love" (*wadd*) 55, 69
"lover, loved one" (*wadd*) 51, 63, 69, 85
waddy "my beloved" 161
wdd f "to be fond of" (*wadda fī*) 165 "

Here, as well as in WiHaI, wdd is given a single meaning, that of "love." But, the

last line of his quotation contains four gross errors. [1] "To be fond of" which means "to have an affection or liking for" is not synonymous of "to love." For the text involved, Hâʾil 165, cf. JaMAR, XIII, p. 171. [2] The "of" of his "to be fond of" is idiomatic and cannot render the preposition f in the Thamudic expression wdd f- which means Love [was] in. [3] The verb wdd being transitive does not govern its complement through a preposition. Only the 4th Ar form sometimes has the preposition ʾilā. It is amazing that two persons, Winnett and Clark (cf. above, p. 146, for his expression "to follow after"), born and raised in English-speaking countries, would both commit the very same error? when using an intransitive verb + preposition as the idiomatic equivalent of a transitive verb, they both consider that preposition as independent from the intransitive verb and, therefore, take that preposition as the rendering of its correspondent in an inscription. [4] In an effort the justify his interpretation, Winnett invents an inexistent Ar formula, "wadda fī" (the 1st form).

In his recent paper, Winnett introduces the idea of "friend" (cf. the reproduction of his text above, p. 148B), which is a secondary meaning of Ar wadda (cf. FrLAL, IV, p. 448, LaAEL, p. 2931B, and even Winnett's favorite SaAAL, p. 1190A). The meaning of "friend" was not mentioned in WiHaI or WiReAES because the translation of "wdd to love, lover, loved one" and his interpretation of the texts did not clash. In the present paper, they d o for Winnett's preconcieved idea on ʾtm forces him to reject the idea of love from wdd. Therefore, not only did Winnett add the meaning "friend" to that of "lover," but he also he retains it to the exclusivity of "lover, ..." in his translations of Hu 126, 263, 317 and 769. It is self-evident that he COULD NOT MAINTAIN the meaning of "love" which HE KNOWS is basic without ACCEPTING in the texts a SEXUAL CONTEXT which he REJECTED FROM THE START as a result of his preconcieved idea on ʾtm.

b - Winnett's theory on his prepositions "nm = lm."

Winnett keeps repeating his erroneous theory on his so-called preposition "nm = lm = By" (cf. Hu 749 and Eut 94) which forces him to arbitrarily cut a phrase into two independent sentences (cf. Hu 317 and JsaT 368). But, due to some hidden logic known to him only, he drops the same interpretation of "lm" which he should have applied to Hu 770 = 775. His opinion was well formulated in WiSLTI (p. 29) as follows:

" The clue to the meaning of *n-m* seems to be furnished by the word *l-m* which introduces so many names in "Thamudic A". It can scarcely be anything else than a dialectical variant of *l-m*, the interchange of *l* and *n* being a frequent phenomenon. "

This opinion was rebuked in JaTS, pp. 42-44, and Winnett's 1981 rehearsal of the same erroneous theory was dealt with in JaMAR, XIII, p. 179.

c - Winnett and his so-called imperviousness of his class "B."

(1) Winnett remarks that

> " it is important to observe that they are all written in a type of script which I have labelled "Thamudic B"[6] or "Najdi Thamudic."[7] "

with reference to both WiSLTI, "pp. 20, 28-34 and pl. X" (p. 239B, note 6) and WiReAR, "p. 205" (p.240A, note 7). Winnett's classification of Tham texts, whether in five groups as in WiSLTI (1937) or only in four in WiReAR (1970) or possibly (who knows?) only in three sometimes in the future, is based on either his inaccurate reproductions of former copies or his own incorrect facsimiles of texts. As demonstrated in JaMAR, XIV, pp. 33-35, to deal with his two classifications amounts to juggle with distorted letter forms.

(2) The classification of his 26 texts into A SINGLE CLASS (cf. above, his "all"), viz. his "B = Najdi" is arbitrary even according to his own rules.

Hu 104 (his no.1), 126 (his no. 4) and 129 (his no.6) are attributed to his "B" on the sole basis of a single letter, ḏ. Hu 114 (his no. 2) is given to his "B" because of ḏ, but the rounded "h" which Winnett corrects to "(m)" because he needs this letter very badly, belongs to his "C" and "D." Hu 125 (his no. 3) is also given to his "B," but the ḏ belongs to his "C" and "D," and Winnett reads it as "h"! In Hu 128 (his no. 5), the ḏ also belongs to his "C" and "D," etc. The value of his "B," "C," etc is no more no less than of a window dressing; regardless of what he put in, Winnett will still do what he wishes and when it is necessary to suit his whim.

(3) Winnett argues as if his division of his "Thamudic B" and its imperviousness to any other of his Tham groups were an epigraphical evidence ... because it suits him now; any other time, he would deny it. However, since his classifications are definitely not an epigraphical evidence, his argumentation is entirely subjective, viz. that which he likes now; consequently, he cannot expect anyone else to consider it as sacrosanct and untouchable as he does.

(4) WiReAR (p. 69) states that

> "I recognized that such a classification was neither complete nor entirely satisfactory. "

But, he fails to give the reference to any pages of WiSLTI where he would have published such an avowal. In this publication, one may find the following statements in

the general introduction to his study, viz.

> " I do not claim to have met with complete success but I am confident that I have been able to establish Lihyanite and Thamudic studies on a much firmer basis than hitherto. " (p. 9).

The real value of these affirmations, however, does not exactly reflect what the author implies in his introduction to the Tham inscriptions, viz.

> " The most important conclusion is that it is possible to divide up the Thamudic texts into at least five groups which differ from one another quite noticeably in the type of script employed as well as in vocabulary and to a certain extent in the direction of writing. " (p. 20).

The apologetic guardedness of the first text gave place to, and is swallowed by, the firm assurance of the second.

(5) As a consequence of his description of his "B," Winnett concludes that

> " We should not be surprised therefore if ʾtm in Thamudic B has a different meaning from what it has in Safaitic or in late Thamudic texts. " (p. 240A).

Nobody is denying the existence of local particularities in letter forms and in some linguistic or religious expressions. All that happens also in all languages; it is the opposite that would be exceptional. Assuredly, a shift in the meaning of a word is always possible, but it must be proven, not merely assumed to suit someone's whim. Furthermore, his affirmation is valueless unless substantiated by the study of all texts involved and not at all by a trumped-up imperviousness invented for the sake of the cause which is all the more unacceptable in that all the rejected inscriptions belong to the same linguistic Ar group. Last, but certainly not least, he speaks of "late Thamudic texts," in in so doing, he simply throws dust into the eyes of the reader and contradicts himself in the process. Indeed, he does not give a single example of those "late Thamudic texts," and HE COULD NOT GIVE ANY because they DO NOT EXIST since he assigns A L L ʾtm texts to his "B."

(6) Three more remarks on WiSLTI's treatment of his "B" are in order.

(a) The no.2 illustration of his "d" on pl. X is twofold, viz. three vertical bars crossed perpendicularly by one horizontal bar and the same with the horizontal bar going beyond the two extreme vertical bars. This sign is reported to be in "Hu 626" (p. 34) = Hu 789d. This text was republished by WiReAES (p. 88, no. "203(d)") where the letter is given the value of "ṯ." It would not have been a luxury for Winnett to update WiSLTI's chart in WiReAR and in the very least, to acknowledge his change of mind.

(b) Winnett's ingenuity of adapting the meaning of a word to suit his translations is remarkable: he gives no less

than five different meanings to his so-called preposition "rm" in his "B" (pp. 29-34), viz. "by": 5 times (p. 29), 3 times (p. 30) and 2 times (p. 31);
"for": 1 time (p. 30); "to": 3 times (p. 30);
"(proceeding) to": 1 time (p. 33); and
"belongs to": 1 time (p. 31).

vdBrIT followed in Winnett's footsteps by giving the same "rm" three different meanings, viz. "par, à, pour" (p. 515B). (c) An important feature of WiSLIT's study of his "B" is his acceptance as n o r m a l of the form "rḏ" for rḏw in JsaT 462 (p. 30) without any explanation or justification. JaSaMA had made it clear that it was not so, and wrote that

"*Le* waw *final n'est pas exprimé ici, pas plus qu'aux n^os* 462, et 380. *C'est une simple omission qui se répare aisément.*" (p. 562, commentary on JsaT 296). This opinion was repeated by LiTS (p. 80, no. 130 = JsaT 462); cf. below, D 2 c (6)).

vdBrIT (p. 545A) also lists as normal the "f. apoc. rḏ" in 12 texts, and the form rḏ² in two inscriptions, both forms being attested in Hu 80. The form rḏ² is found in Hu 633 (cf. Saf rḏw = rḏ² in JaMAR, XIII, p. 272B), but the so-called apocopated form "rḏ" is not demanded by any text because all the texts invoked yield satisfactory meanings without being tampered with by the restitution of one inexistent letter. I also find it offensive to see anyone accusing the Tham writers of having repeatedly apocopated the name of the well-known deity rḏw by carelessness or negligence or any other reason.

Here follows the list of the texts given by vdBrIT (p. 545A) and the references to my publications where they were dealt with.

Hu 74, cf. JaMAR, III, p. 132, note 54.

Hu 80a, cf. l.c., V, p. 50: the reading of "rḏ(w)" is justified because the left side of the w is clear on the facsimile.

Hu 80b, cf. l.c.; there is no "rḏ²" in the text.

Hu 106, cf. my Minaean Inscriptions Published as Lihyanite, Washington, 1968, p. 129, note 83, where it is noted that the letter is clearly written in the facsimile published in Bulletin de la société de géographie, 5 (1884), p. 295, pl. 3, no. 38.

Hu 144b = But 175, cf. JaMAR, VII, p. 136, note 100.

Hu 227c, cf. l.c., V, p. 62.

Hu 633, cf. below, D 2 c (6).

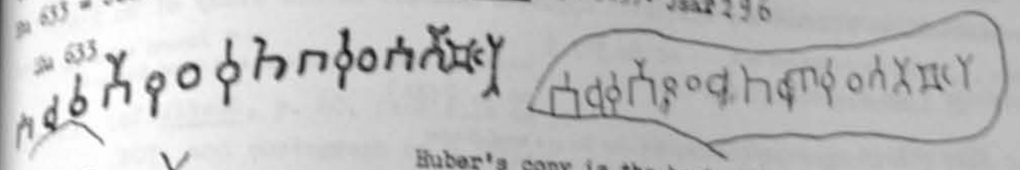
Hu 803, cf. JaMAR, V, p. 103, note 85.

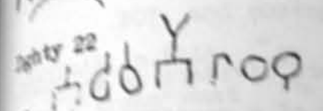
Hu 474, cf. below, D 2 f (1).

Hu 694, cf. my Minaean..., pp. 115-116, no. 62.

Hu 804, cf. l.c., p. 100, note 57.

JsaT 380, cf. immediately below. JsaT 462, cf. immediately below.
JsaT 563, cf. JaMAR, V, p. 102, note 74.
There follows the study of the three remaining texts which have not been dealt with before.
Hu 633 = JsaT 296 = Dghty 22 (cf. vdBrIT, p. 325). JsaT 296

Hu 633 

Dghty 22 

Huber's copy is the best with the exception of the initial letter the form of which is contradicted by the two other ²'s, and the antepenult letter which has the same awkward form as in Dghty's copy. These two letters are correctly written in JaSaMA's copy.

The text may be read as follows: hrḏ² s(w)q bk q^cy² qds 0 Ruḍâ²: Sâ(wi)q has cohabited with Qâ^caya² [during a period of] purity. — s(w)q: vdBrIT transcribes the word as "s^cd"(as HaNKIP, 9 [1901], p. 350, and JaSaMA) in spite of the fact that the two complete copies have both "s^cq." It is much easier to correct ^c to q than q to d. Cf. the verb in Ḫâ²il 139 in JaMAR, XIII, p. 169. — bk, e.g., Hu 158. — q^cy², cf. Ar qa^ciya "to have a turned-up nose" (cf. SaAAL, p. 799B). — qds, cf. Ar quds and qudus "purity" (cf. LaAEL, p. 2497A): the noun may be understood as a temporal adverbial complement.

JsaT 380: 380

vdBrIT reads the text as follows: "b-r-ḏ m-s-^c-ṭ By Ruḍâ! Nas^caṭ" (p. 316).— The text may be read as follows: brḍm s^cṭ Barḍum has taken nose medicine [here]. — brḍm, cf. brḏ in Hu 803. — s^cṭ, cf. Ar sa^caṭa "to pour medicine into one's nose" (cf. LaASL, p. 1364B).

JsaT 462: 462 (p. 281)

vdBrIT reads "h-r-ḏ n-ṯ-r n-m ḏ-b-n 0 Ruḍâ, aide pour Dibân," a reading which he himself says comes from both WiSLIT (p. 30) and LiTS (p. 80, no. 130); the latter says that

"Das ! im ersten Worte ist wohl nur aus Versehen weggelassen, im Original oder in der Kopie," and, therefore, he transcribes it as "hrḏ[w]." In so doing, LiTS clearly shows that, in his opinion, the form "rḏ" is irregular. — The text may be read as follows:

hrḏ nṯr nmṭ bn Haraḍ has helped Namiṭ [who] had stopped [here].

hrḏ, e.g., JsaT 563. — nṯr, e.g., WiTa 16/1. — nmṭ, cf. the verb in Hu 647.

d - Winnett and the word dividers.

There are nine texts in JaTV where vertical strokes are understood as word dividers, viz. Hu 125, 126, 238b, 263, 317, 769, 801, JsaT 368 and 576. This feature is not mentioned in Winnett's study of Hu 126, 263 and 769, but it is pointed out without any special opposite comment in his study of Hu 238b ("Jamme ... taking the vertical line as a word-divider" on p. 241B), of Hu 317 ("Jamme takes the vertical line after *ʾrm* and *wdd* as a word-divider" on p. 242A) and of JsaT 576 ("Jamme takes the vertical line after *ʾrm* as a word-divider rather than the letter *n*" on p. 244A).

On three occasions only does Winnett speak his mind on the word dividers as such. JaTV's reading of Bu 125 (p. 292A) includes the understanding of a vertical line a word divider once. Winnett's categorical reaction is this:

"Jamme ignores the fact that in Thamudic B the short vertical line always has the value *n* and takes it as a word-divider" (p. 241A).

I wish to draw the attention of the reader on the adverb "always" in Winnett's statement.

My reading of JsaT 368 includes the same interpretation twice. For Winnett, my reading "cannot be accepted because in Thamudic B the vertical line has the value *n*." (p. 243A). Note that the adverb "always" has disappeared.

IN SPITE OF THE TWO PRECEDING STATEMENTS, and especially of THE FIRST (cf. the adverb "always"), Winnett's reading of Hu 801 (p. 243A) shows his understanding of a vertical line as a word divider FOUR TIMES, and his commentary in the left column of the same page as that of his second affirmation quoted above, states that "The word-divider is used very sparingly in Thamudic B inscriptions" (p. 243A).

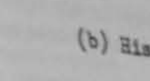
(1) The most obvious objection is this. It is difficult for an author to contradict himself more blantly in the very same paper. This example also is an unmistakable proof that Winnett does not really understand the meaning of the words he uses and is above anything else bent on building arguments strictly for the sake of the cause. For the so-called impervisousness of his "B," cf. already above, pp. 150-152. Finally even were his divisions acceptable, his expression "very sparingly" is vague enough to cover three or four more examples beside those of his theory.

(2) The author states that

"Apart from Hu 801, I have noted it only in six texts copied by W. L. Reed and myself in 1962 and 1967" (p. 243A).

(a) Preliminary remark - The number and contents of the errors and misunderstandings already noted above and also all those contained in the following pages as well as the innumerable criticisms previously leveled against his argumenting method and his work exclude any objective value from his subjective testimony manifested in his preceding statement.

(b) His first reference is with 23 (cf. WiReAR, p. 80, facsimile on p. 207, and photograph on p. 216; cf. JaRWiReAR, p. 524A, where "ʿtrsmn" must be corrected to "ʿtrsm").


23

"23 Phot. W 2:22 and copy. Jawfian script. — To the right of nos. 21 and 22. Pls. 3 and 12.

ʾrḏw wnḥy wʾtrsm sʾdn ʾl wddy

O Rudā and Nuḥai and ʿAttarsam, help me in the matter of my love."

[1] Winnett's transliteration does NOT register any of the three word dividers as if they were inexistent.

[2] He affirms that "A word-divider is employed between the divine names." This statement is erroneous: there is a word divider after each divine name, not only "between" the three names.

[3] Winnett contradicts himself openly. He repeatedly restricts his argumentation to his "Thamudic B." Yet, the edition of WiTh 23 declares (cf. above) to text to be written in his "Jawfian script" (cf. my remarks in JaRWiReAR, pp. 523B-524A), "a script which resembles Taymanite" (cf. WiReAR, p. 73). Therefore, he introduces in his argumentations on "B" texts which do NOT belong to his "B" and which are "presumably of much the same date" as that of his "Taymanite." Therefore, in good logic, he cannot exclude both his "Jawfian" and his "Taymanite" from his argumentations. It should be added that, although placing his "Jawfian" next to his "Taymanite" in his chart (cf. WiReAR, p. 205), Winnett categorically states in his introduction that "In the present work the Thamudic texts are classified into three groups: Najdi (my former "Thamudic B"), Hijazi ("Thamudic C and D"), and Tabuki ("Thamudic E")." (cf. WiReAR, p. 70). Thus, his "Jawfian" is squarely eliminated from his Tham. Yet, the words of the texts written in his "Jawfian script" resurface again in the glossary, but in the wrong place, viz. in the "Thamudic" section (cf. WiReAR, pp. 193-196). The preceding facts clearly indicate that Winnett can do nothing but mess up things ... unless, of course, he does not understand the words he uses.

(c) His last five references a-
re Gā'il 63, 64, 66, 68 and 138 (cf. Berytus, 22 [1973], pp. 71-72, facsimiles on
pl. 4 and 10, and photographs on pl. 5 and 11; for the republication of those fi-
ve texts, cf. JaOAH, XIII, pp. 161 and 169).

[1] Those texts should be classified in his "Jawlam." But, this is excluded by his statement according to which "The majority are in the Najdi type of Thamudic; the others are in the Hijazi and Tabuki types of that script." (cf. Berytus, l.c., p. 63).

[2] Each sign of Gā'il 63 is interpreted as a letter in the editor's transliteration. Therefore, even for Winnett himself there is NO word divider in the text. But, now he affirms that there is at least one word divider. Here again, he contradicts himself.

[3] The word divider of both Gā'il 65 and 66 are, respectively, a normal stroke and a short stroke; the word divider of Gā'il 68 also is a short stroke, and that of Gā'il 138 a normal stroke. It must be emphasized here again that Winnett made the following statement already reproduced above, in the center of p. 154, viz. "the short vertical line ALWAYS has the value ŝ" (the capitalization is mine).

(3) In Tham the word divider is represented as a vertical stroke, normal or short, as in Lih where the line is sometimes reduced to a very short line (e.g., JaL 145k), two dots (e.g., JaL 145r) and even a single dot (e.g., JaL 145a).

In Tham, are also used as word dividers

one dot: e.g., once in JsaT 509, 515, 575;

twice in JsaT 547, JaT 38j; cf. also Hu 521 = But 717 where each letter is isolated by one dot on each side;

two dots placed vertically: e.g., once in JsaT 396, 450; and

thrice in Hu 696;

one short stroke: e.g., once in JsaT 432, 485, 556; and

twice in JsaT 428, 553, 560;

one short stroke and one dot: e.g., in JsaT 568; and

one short stroke and two dots placed vertically: e.g., JsaT 426, 505 and 568.

e - Winnett's rejection of the Ar linguistic group.

Winnett never justifies his rejection of what appears the most natural etymology of Tham ʾtn from Ar ʾatama of Saf ʾtn. The reader will see further (cf. D 6 c,d) that this a priori and gratuitous rejection is in turn rejected by the same Winnett who quietly goes back to Ar ʾatama as in Saf: this is the most basic and

spectacular about-face of his Tham section.

f - Winnett and the invocation to a deity.

Winnett claims that my interpretation of Tham ʾlm understood as laf [illegible] "is completely misleading" (p. 239, summary) or "impossible" (p. 240a) because of what he calls" the pattern of the inscriptions" (p. 240a). And he explains that so-called "pattern" as follows: "When men call upon the gods, it is for the purpose of asking some boon; hence the verbal form which follows the divine name is almost of necessity an imperative. This consideration makes it impossible to believe that Tham 'ow has anything to do with previous sexual encounters. The authors of these inscriptions were not calling upon the gods to reform them that they had had sexual relations with Sin-and-Sin, although that is what Jamme's translations would have us believe" (p. 240a).

The premise stated in his initial statement obviously is fallacious (cf. already above, pp. 157-158): humans also call upon their gods in exclamative or swearing sentences to express or underline their feelings or to summarily describe events, favorable or not, in which they find themselves involved or which they witness. Such is also the case in Ar; cf., e.g., katabtu bi l-qalami Calamo scripsi, bi ʾllāhi Per Deum." in FrLAL, I, p. 77A; cf. also J. Peterson, Der Eid bei den Semiten (Strassburg, 1914), who lists on p. 14 bi as the most frequently used preposition in swearing formulas. Other famous examples of this usage is Latin "Per Jovem!" which was typically used to express surprise or agreement; the Greek expressions κατὰ τοῦ θεοῦ By God!, πρὸς τῆς Ἑστίας By Vesta!"; the French "Parbleu! = pardieu" or the English "By God!" which have become swearwords to underline and strengthen a statement. Nobody in his right mind would dare to claim that the word or phrase immediately connected with the above-quoted exclamations would in fact attribute the paternity of its contents to God. Furthermore, the reader is flabbagasted to find in WINNETT translations consisting of mere statements contradictive of Winnett's two above-mentioned affirmations, whether the divine name is introduced by b- or b-; e.g.,

Hu 707/2: "By S-h-y he cursed" (p. 30); cf. JaMAR, VII, p. 133, note 56.

Hu 719/2: "Up! Radu, the fighter" (p. 30). Such a translation is ridiculous; here again, he isolates l.2 from l.1; cf. JaMAR, VII, p. 149, note 176.

Hu 800/1: "O Allah, lacking is vengeance to ʾUmayat" (p. 30). What a strange wording! Here again, he isolates l.1 from l.2; cf. below.

JsaT 332: "By D-th-n! the people of a caravan (proceeding) to Dedan" (p. 33); here again, l.1 is isolated from l.2; cf. below.

JsaT 551: "By S-h-y, they are filthy! By ʾadabʾil" (p. 29); cf. below.

Even in a more recent publication, WiReASS (pp. 53-100), the reader still finds other examples of the same, viz.

ʒâʾil 57: "By DTN loathing attacks a lover" (p. 49). It is difficult not to qualify such a translation as stupid; cf. JaMAR, XIII, p. 158.

ʒâʾil 69: "By NHY is the jealousy of love (or a lover)" (pp. 72 and 97, note 80). Same remark as above; cf. JaMAR, l.c., p. 161.

ʒâʾil 80: "O DTN, I have a disease (?). By Hutaim for Tais (or a he-goat)" (p. 73). JaMAR, l.c., p. 153, gives the preceding translation as an example of a "wild, fancy imagination and a lack of good common sense" because "a human being engraved a message on a rock for the benefit of a ... 'he-goat'!;" cf. l.c., p. 163.

WiReASS, however, is a major turning point in the evolution of Winnett's thinking which ultimately led him to write the present paper. His new theory, or better said, his new systematization is based on his erroneous assumption that the preposition b- introducing a divine name always mean "By" in the sense that the deity is attributed whatever is mentioned in the rest of the text, e.g.,

Hu 474: "By Rudā are we" (p. 96 and note 57). Cf. below.

Hu 215: "By NHY are slave girls. By ḤWDT" (p. 97 and note 76). Winnett gratuitously corrects the first and fourth letters to find his "hnhy;" cf. JaMAR, VII, p. 132, note 72.

Hu 707/2: "By NHY is the flying into rage" (p. 97 and note 79). Winnett does not even refer to his WiSLTI's translation quoted above (p. 157).

Hu 792: "By NHY, through thee is manifested the passion of the poet" (p. 97 and note 81); cf. JaMAR, XIII, p. 239.

Beside the fact that the last two translations are stupid and the last one is even incomprehensible, it is regrettable that Winnett did not apply to his translations his own judgment according to which "that [= such translations as his first two] can hardly be thought of at this stage of Arabic religious development." As stated at the beginning of this section, what is more natural to understand those texts, as it has been done for centuries, as composed of an exclamation expressed by b- or b- + divine name followed by a statement on a matter which, although possibly trivial to us - but we are not the authors of the texts - must have been of special concern for the author when he wrote his text! Furthermore, it should be strongly emphasized that the verbs "are" and "is" are inexistent in the texts and should have been put between brackets; they are Winnett's inventions for the purpose of rendering his translations more palatable.

Here follows the study of the five texts mentioned above, viz. Hu 474, 657, 792, 800 and JsaT 551.

Hu 474 = Eut 656.

Hu 474

Eut 656

WiReASS's reading is "b(r)ḏ(w) nḥn By Rudā are we" (p. 96 and note 57); the translation is already quoted above (p. 158).

I must repeat here that the verb "are" does not exist in the text. — The left angular corner of the initial letter of Huber's copy leads quite naturally to the letter form of Euting's copy. — Euting himself declares the text to be "deutlich." Winnett does not apparently value either the honesty or the ability of Euting who certainly could reproduce very nicely what he saw; cf., e.g., the 1884 calendar entitled Reise nach Innerarabien 1883/84. Aquarelle aus den Skizzenbüchern des Orientalischen Julius Euting nach den Originalen im Besitz der Universitätsbibliothek der Eberhard-Karls-Universität Tübingen (Tübingen).

Winnett willfully spurns and tramples such an overwhelming testimony into the dust, viz. he corrects no less than three letters out of seven in order to make the text say what he himself wants to find in it. It would be difficult to find a better example of contemptuous distortion of a well-documented information.

HaNKIP (9 [1901], p. 344, no. 1) knew Huber's copy only, but having no preconceived idea as Winnett, Halévy correctly read the copy as "bl ḏʿn nḥn" without giving any translation. — JaMAR, V, p. 79, reads the text as follows:

kl ḏʿ nḥn Kall has frightened Naḥḥân.

Hu 657 = JsaT 331+.+332 (cf. vdBrIT, pp. 332-333).

Hu 657

331 JsaT

332

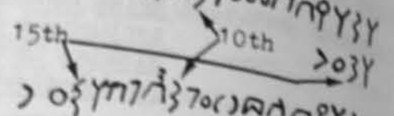
On p. 33, WiSLTI retains JaSaMA's copy preferably to Huber's and reads JsaT 332 as follows: "b-d-th-n ʾ-h-l ʿ-r n-m d-d-n" and his translation is given above, p. 157, bottom. WiSLTI's pl. IV, "group 5", practically distorts every letter of the text. By ignoring l.1 of Huber's copy, Winnett induces his reader into error by formulating a translation where ddn can only have the meaning of the famous city of the same name although Huber's copy makes it clear that ddn is a personal name (cf. also G. Ryckmans and LITS [pp. 86-87, no. 142] who retains JaSaMA's copy, but refrains from suggesting any interpretation of the text). Finally, Huber's copy must be preferred to JaSaMA's where not only l.2 is missing but also the end of JsaT 331 is unclear, and the copy of JsaT 332 contains a dextrograde "r" which most probably is the left edge of a feature of the stone with a shell-like (hence, the "ʿ") design in its center.

The text may be read as follows

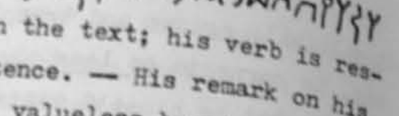
nm ddn	1	Has slept [here] Dôdân.
nhy ʾnh ġrm	2	ʾAnih has forbidden Ġânim.
bdtn bhl ʿn mddn	3	O Dattân! ʿAwn was free from Dôdân.

ddn, also a personal name, e.g., in JsaT 394. — nhy, cf. Ar nahâ (i) "vetuit, interdixit" (cf. FrLAL, IV, p. 347A). — ʾnh, e.g., Saf CIS 4145. - ġrm, e.g., Gheyta b. — bhl, JsaT 252 in my Minaean Inscriptions..., p. 144. — ʿn, Hut 748.

Hu 792 = 809.

792 

15th 10th

809 

WiReAES (p. 97 and note 81) reads the text as follows: "hnhy bk mʿln ʾlb hšʿr" and his translation is reproduced above, p. 158, center. — Here again, there is no verb "is" in the text; his verb is restored to give his sentence the look of a normal sentence. — His remark on his "šʿr" reads as follows: *"the first sign has practically the same form as the s of sly, but in 646 (19) it is clearly differentiated from it, a fact which makes the identification with š highly probable."* (p.97,note 81). This remark is valueless based, as it is, on the wrong parallel. The true parallel is between the 10th and 15th letters of each copy: in Hu 792 and 809, the 10th letter is composed of five and four elements, respectively; the 15th letter of four and five elements, respectively. Thus, the two signs are interchangeable and must be given the same value. — The text may be read as follows:

hnhy bk mʿl nʾl bh nʿr

O Nahî! Bawk castrated Nâʾil [who] cohabited with Naʿir.

For the reading of the second, third and fourth names, cf. JaMAR, XIII, p. 239.- bh, e.g., Hu 208. — nʿr, e.g., Saf JaS 33. Hu 800.

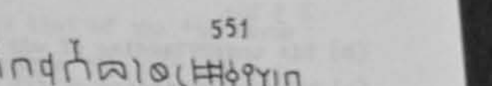
WiSLTI (p. 30) deals only with l.1 and reads it as follows: "h-ʾ-l-h ḏ-ʾ-l n-ḳ-m n-m ʾ-m-y-t" and his translation is quoted above, p. 158, center. — Winnett's facsimile on his pl. IV, "group 2," distorts the three ʾ's and the two m's of the text. — Here again, the verb "is" is inexistent in the text. — His traslation of "ḏʾl" as "lacking" is a fiction. — The text may be read as follows: hʾlh ḏʾl nqm ʾmyt 1 O ʾIlah! Was abject Nâqim [who] calumniated ʾAmyat.

nm kht 2 Has slept [here] Kahat.

L.1: same reading of the letters as in WiSLTI. — ḏʾl: LiTS (p. 82,no. 136,where

l.2 is not dealt with) retains the reading of the initial letter as "d" but without excluding the possibility of the letter being ḏ. Cf. Ar ḍaʾula "to be,become, small in body, thin and despicable, abject, or ignominious" (cf. LaAEL, p.1760A). — ngm, Hu 780/3. — nm, e.g., Hu 81/2. — kht: already HaNEIP (10 [1902], p. 64, note 10)suggested the doubtful reading of ḫ. Without mentioning HaNEIP, Littmann reads the letter as ḫ and sees in kht "ein Diminutiv von k-ḫ" (cf. BiOr,9[1952], p. 220). Even were kht derived from the root khh (cf. also HaIC, p. 495), it would still not be a diminutive, but a qtlt (not a "qtylt") derivation. The name derives from the root kht; cf. Ar ʾakhat "short, as an epithet applied to a man" (cf. LaAEL, p. 2594C).

JsaT 551.

551 

WiSLTI (p. 29) reads the text as follows: "b-n-h-y k-dh-r-w n-m ʾ-d-bʾ-l," and his translation is reproduced above, p. 157, bottom. In the author's copy of the text on his pl.IV, "group 2," none of the letters is drawn accurately. — His reading, which was endorsed by vdBrIT (p. 304) is unacceptable: the 6th letter is ṯ, not "dh;" the 10th letter is ġ, not "n;" and the subjects of the phrase, "they," remain undetermined. — The text may be read as follows:

bnhy qṯr wġm ʾdbʾl

By Nahî! Qâṯir has treated ʾAdabʾil with violence.

qṯr, cf. Ar qaṭara "stillavit, guttatim fluxit aqua" (cf. FrLAL, III, pp. 463B-464A). — wġm, cf. Ar waġama "violenter tractavit aliquem" (cf. l.c., IV, p.486B).

8 - The contexts of the ʾtm inscriptions.

In the arsenal of Winnett's arguments the most high-sounding and impressive one is based on "the contexts" (p. 239A) or "the pattern of the other ʾtm-inscriptions" (p. 242A). There cannot be any doubt that, when he mentions either of these two expressions, Winnett is convinced of giving the quietus. But, the most important question is to accurately determine the characteristics of that pattern.

(1) Winnett invokes that finishing stroke in four cases, viz. (a) his rejection of Clark's opinion on Saf ʾtm (cf. below, D 3);

(b) his division of Hu 402 into two different texts (cf. below, D 7 k (2));

(c) his rejection of a possible interpretation of his "dʿn" in Hu 627 (cf. below, D 7 l (3)); and

(d) his interpretation of Hu 749 (cf. below, D 7 h (5)).

Since Winnett does not explain himself in the first case, it is impossible to know what he is referring to. In both the second and fourth cases, his reasoning is erroneous. Finally, in the third case, he is flailing the air because his "d^{c}n" is not the correct reading.

(2) In four other cases, however, Winnett ignores the nature of the contexts and suggests solutions opposed to them, viz.

(a) his acceptance of the Hebrew adjective tamîm in relation with ʾtm (cf. below, D 5 b);

(b) his understanding of wdd mentioned in the ʾtm texts (cf. above, pp.147-149)

(c) his understanding of ʾlh of Hu 627 (cf. below, D 7 l (1)); and

(d) his acceptance of b introducing the author's name in Hu 114 (cf. below, D 7 c (2)).

3 - Winnett's rejection of V.A. Clark's opinion in Saf.

It is immediately after his long statement quoted at the beginning of D 2 f (cf. above, p. 157) and at the beginning of his last introductory paragraph devoted to his Tham presentation that Winnett at long last condescends to deal with V.A. Clark's opinion on Saf ʾtm. His judgment is as follows:

" the contexts in which this expression occurs in Saf do not support his interpretation. " (p. 240A). This statement is already reproduced above, p. 148. Not a single word of explanation is given, and the reader is left completely in the dark as to how, what and why. Furthermore, since Winnett speaks of Saf only, the three lines are completely out of place exactly as "un cheveu dans la soupe" of his Tham presentation.

4 - Winnett's rejection of A. van den Branden's opinion.

Winnett then turns his attention to van den Branden's opinion :

" Van den Branden[10] takes Tham ʾtm to be the imperative to tmm, IV (cp. Ar tamma, "to be or become complete") and translates "rends parfait." (p. 240A) and his not 10 refers to vdBrIT, "pp. 71, 518, etc."

vdBrIT's p. 71 (commentary on But 94) presents his opinion in a very short statement, viz. "ʾtm, impert. IV f. de tmm," which is repeated in two complementary places, viz. "ʾtm, cf. tm" (p. 511B) and "tm, IV, rendre parfait" (p. 518B). Winnett's reason for rejecting vdBrIT's interpretation is most astounding; he writes that van den Branden's

" rendering is that it suggests a striving after moral perfection, something that can hardly be thought of at this stage of Arabian religious development. " (p. 240A).

It is a truistic statement or a "lapalissade" that "perfect, perfection" are said of everything, material or not; therefore, he who restricts them to things "moral" shows that he does not understand the words he uses.

In note 10, Winnett says that

"For a brief survey of the attempts that have been made to find a satisfactory meaning for ʾtm, see M. Höfner's review of vdB, IT, in Orientalia NS 23, 1954, p. 316. She herself seems to favour vdB's interpretation. "

Winnett misunderstood M. Höfner's German. After giving the list of the ʾtm texts dealt with in vdBrIT to which she adds "But, 94," she mentions "Hu 129" which is misprinted as Hu "127" in vdBrIT (p. 518B). After noting that ʾtm is understood " bisher immer als IV. Form von tmm ," she suggests that " Es wäre zu überlegen, ob man nicht zur Deutung dieses Wortes von arab. 'atamma " vereinigen, bleiben " (Lane, s. v.), asa. ʾtm " sich anschliessen, sich einigen ", " and she gives the SA and Soqoṭri parallels. There can hardly be any doubt that this is the opinion she prefers because it is the most obvious etymology. But, she continues saying that "Unter der Voraussetzung einer Wurzel tm ...

... so heisst tm wohl am wahrscheinlichsten " gib Gelingen " (so Littmann); " mache vollkommen " (v. d. Br.) scheint dem Geist dieser Inschriften kaum recht angemessen zu sein. "

For Winnett to say that "she herself seems to favour vdB's interpretation" is to give the proof that he understood the opposite of what Höfner wrote because for her, vdBrIT's interpretation is "hardly correct." Finally, I wish to draw the attention of the reader to the fact that the reference to Höfner's exposé was dropped from the end of JaTV's note 1 (p. 290A).

5 - Winnett's exposé of his theory on ʾtm.

a - Winnett has finally arrived at the crucial section of his introduction, viz. the formulation of his theory. However, if the reader wants to understand his introductory statements he must go beyond the lines devoted to Clark's and van den Branden's opinions and back to his statement reproduced above, p. 157. It is with the last sentence of that quotation that his following introductory statement ties in, viz.

" It is much more likely to express a desire to be made complete or whole physically, i.e., to be restored to health. " (p. 240A-B).

Were Winnett able to set aside even for one second his preconcieved idea, which he wants at all cost to inject into the texts, viz. "to restore to health," he

(d) his interpretation of Hu 749 (cf. below, D 7 n (5)).

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Were Winnett able to set aside even for one second his preconcieved idea, which he wants at all cost to inject into the texts, viz. "to restore to health," he

might see that his above-quoted statement is a vicious circle and makes no sense at all. None of the authors of these texts claims to be sick or physically defective. What reason might, thus, have any of them to beg his god for something he already has? The bedouins are too realistic to waste their time to such nonsense. Furthermore, Winnett does not even see that he is contradicting himself. He wrote in the left column of the very same page 240A that "when men call upon the gods, it is for the purpose of asking some boon" (cf. above, p. 157, for his quotation). Where is the "boon" to be requested or to be gotten when the caller already has what he is supposed to ask for?

b - Winnett then reveals the origin of his "etymological support." It is not the self-evident etymology from Ar ʾatama; it is not any of the languages of the Ar group, whether Saf or "even late Thamudic," though the speakers of those languages lived next to the writers of the texts and sometimes with them in the same communities. It is Hebrew, a language spoken by foreigners whom all those Tham persons never had any contact with. And to make his case even worse, he calls upon a very special adjective, tamîm, viz.

> " Etymological support for this interpretation is to be found in Hebrew where the adjective *tāmīm* is used to describe an animal that is perfectly healthy, "without blemish" (p. 240B).

This Hebrew adjective means "without blemish, defectless," a prerequisite condition to be met by all would-be sacrificial animals to be slaughtered in the temple of Jerusalem. But, the Tham texts deal with human beings only, not at all with a temple or animals whether to be slaughtered or not. Furthermore, the Hebrew adjective basically refers to external appearance and not directly with health. Surely many of those animals were rejected for some reason which had nothing to do with health , e.g., a wart on the sheep's underpinning.

c - Winnett's conclusion is the very last phrase of his introduction of the Tham section of his paper, reads as follows:

> " In the light of this Hebrew usage it is highly probable that in Thamudic B *tmm*, IV, had the same or s similar meaning. "

Here again, where is the proof to substantiate his claim of "highly probable" except his fanaticism of applying his preconcieved idea at all cost? In other words, Winnett repeats the very same error which he had condemned in Clark's theory.Clark had surrepticiously introduced into the facts the foreign ideas of "being wounded" and "to finish off." Similarly, Winnett injects into the inscriptions the foreign ideas of "being healthy" and "to restore to health."

d - Since, as said in the preceding paragraph, Winnett's conclusion is the very last sentence of his introduction of the Tham section of his paper, the reader concludes that Winnett has said all that he wanted to say ... It is quite true: Winnett does not add a single word to his exposé of his theory discussed above: he said all that he wanted to say. However, this does not mean the end of the line. Indeed, as he has contradicted himself so often in the past, he does it again in a most spectacular way in the middle of his paper ... in Hu 749 (cf. below, D 7 q). But, Winnett does it so quietly and presents the case in such casual a manner that three facts are beyond the shadow of a doubt:

(1) Winnett was fully aware of his about-face;

(2) Winnett did not wish to explicitely alert his reader about his volte-face; and

(3) Winnett hoped that his reader would not realize the full impact of his reversion.

In his study of Hu 749, his no. 13 (p. 242B), ʾtm is translated as "mourning." This is a turning back to his interpretation of Saf ʾtm; and his commentary on the text, 7 and 1/4 lines long, contains only this on that so important matter: " For ʾtm, "to mourn," see p. 1" of his paper. No reference is made to his previously printed statements: "we should not ... late Thamudic texts" (p. 240A); cf. above, p. 151, and "in Thamudic B ... similar meaning" (p.240B); cf. above, p. 164, bottom).

All that self-contradiction and tortuously deceiving argumentation is possible only in an author exclusively interested all through his paper in both arguments built up for the sake of the immediate cause and his preconcieved theory at the expense of everything else, inscriptions, his own writings and the writings of anybody else (cf., e.g., M. Höfner's remarks; cf. above, p. 163).

6 - Winnett's fancy application of his theory.

a - Winnett's handling of ʾtm also is very special. According to his paper,"ʾtm" as an imperative is found in nine texts, viz. But 94; Hu 104, 114, 129, 402b; 753, 801 and Ph, 271z;

"ʾtm" as a noun is in Hu 749 (cf. just above);

"ʾtmn" is either the energetic imperative in 15 texts,viz. Hu 125, 126, 128, 238b, 263, 317, 691a/6, 769, 770=775, 796; JsaT 335, 368, 576 and Dghty 47(4);

or the normal imperative + -n, the personal pronoun in Ph 367k; and

the two imperatives "ʾtm" and "ʾtmn" are seen in Hu 627 where only one single word separates one from the other.

b - Winnett n e v e r justifies the use of the energetic form of the imperative in one text and not in the other and especially when they appear together in the very same short text Hu 627. No justification can be found because there is nothing in the inscriptions which might even suggest an imperative form.

c - Winnett refers to LiDTI's interpretation (p.42) of "ʾtmn" as the energetic imperative of tmm, IV (p. 241A) and laments saying that "Later scholars failed to heed his suggestion." Each scholar has to speak for himself. As far as I am concerned, Winnett's statement is erroneous: I never found a single text demanding such a suggestion. On the contrary, it always appears to be used as an easy way out difficulties, e.g., as in Hu 627 (cf. below, D 7 1). Following into the footsteps of vdBrIT's who frequently uses expressions, such as "sʿdn aide-moi!" and "nqm ʾenge-moi!," an interpretation I have never found imposed by the texts, Winnett translates ʾtmn as "restore me to health" in Ph 367. But, when such an interpretation does not fit, then "ʾtmn" becomes an energetic imperative. And, here again, when the imperative meaning does not fit, then "ʾtm" becomes a simple noun interpreted -horribile dictu! - as in his Saf!!!

d - Winnett did not have very long before facing a deadly dilemma in Hu 749 (cf. above, p. 165). He could not keep intact and alive his record on Tham ʾtm and his theory on the texts starting with b-divine name. Instead he found himself cornered and caught between the anvil and the hammer both of his own creation, and his snare was so unyielding that there was no way out without rejecting one of his two well-orchestrated and trumped-up theses. He chose to contradict himself by sacrificing the first one, viz. the meaning of Tham ʾtm.

7 - The texts.

a - Preliminary remarks.

(1) Winnett's fidelity of reproducing quotations from other authors is regulated by his own habits. A good example is his copy of long vowels, such as "â" and "Î" of vdBrIT's, JaTV's and JaMAR's texts; they are constantly reproduced as "ā" and "Ī." In his quoting JaTV's translation of Hu 125 (p. 241A), he erroneously writes "O Kāhil!" isntead of "O Ka(hi)l!"

(2) In his first 10 texts, his nos. 1-10,

ed in Hut 94, his no. 19, Winnett's bibliography lists two of my publications, viz. JaTV and JaMAR, V. Not only the date of the second is not given on p.244B or anywhere else, but also and more importantly he fails to inform his reader that JaMAR, V, where I published all Euting's copies of texts on 47 plates comprised of 110 sections from the author's "Reisetagbuch" itself, contains the revision of some of my former translations on the basis of the new documentation. This means that JaMAR, V, supersedes any former paper, including JaTV. Winnett does not understand such simple a situation. (a) He still quotes both publications in his commentary on Hu 104, as if they were of equal value.

(b) He does not say where the translation he reproduces comes from in his commentary on Hu 125.

(c) He even reverses the chronological order of the two publications in his commentary on Hu 126 and in both the introduction and commentary on Hut 94.

(3) winnett's sigium "Ja MIL" (p. 242A, commentary on Hu 265) is neither mentioned nor explained in his list of abbreviations (p. 244). (4) The superficiality of winnett's working method is also exemplified by his division of the letters in five texts, viz.

Hu 128: "ʾtmn ddy;" Hu 317: "ʾtmn w(d)d;" Hu 789: "ʾtmn wdd;"
Hu 796: "ʾt(m)n ʿmn;" and Dghty 47(4): "ʾtmn gwl."

The division of the letters as "ʾtm nddy," "ʾtm mw(d)d," "ʾtm mwdd," "ʾt(m)n ʿmn," and "ʾtm ngwl," respectively, certainly is as plausible as his. Obviously enough, he never justifies the choice of his readings which are all the more arbitrary in that the alternative readings would not in the least endanger or jeopardize his theses.

(5) Due to the information given in my study of JsaT 409 (cf. below, D 7 y (2)) on Philby's copies, which confirms - as if it were needed - my previous stand on those copies, I exclude from this study Philby's copies nos. 271z and 367k, which Winnett deals with in his last two nos. 25 and 26 (pp. 244A-B and 244B, respectively).

b - Hu 104 = HuIR 40 = Hut 108 (pp. 240B-241A).

Hu 104: 𐩣𐩤𐩥𐩦𐩧 𐩨𐩩+𐩪𐩫𐩬
HuIR 40: 𐩣𐩤𐩥𐩦𐩧 𐩨𐩩+𐩪𐩫𐩬
Hut 108: 𐩣𐩤𐩥𐩦𐩧 𐩨𐩩+𐩪𐩫𐩬

(1) Winnett fails to indicate that he copied the identification of the two copies of Huber from vdBrIT (p. 78).

(2) Winnett's discussion of the third letter from the right ignores JaMAR's reading (p. 240B), refers only to JaTV(p.240B), but reproduces it at the end of his commentary (p. 241A), viz. totally out of context. He reads it as "ġ" and, therefore, he finds the divine name "yġ" and declares this:

" I suggest that YĠ is an abbreviation of the well-known Arabian god-name Yaġūṯ " (p. 240B).

(a) Let us first take up this comment on his suggestion "yġ" = "yġṯ." There is no reference at all to the name yġṯ. T.Fahd declares that this deity is a"Divinité du groupe tribal de Maḏḥiğ, adorée au Yémen sur une colline (3) portant le nom de la tribu.

A la veille de l'Islam, le culte de Yaġûṯ était encore assez vivace." (cf. Le panthéon de l'Arabie centrale à la veille de l'hégire, Paris, 1968, pp. 191 and 193).

The divine name yġṯ alone is not attested epigraphically. — Winnett accepts a Tham religious dependency from another Ar pantheon, a position which is inconsistent with his stand on the aloofness of his Tham "B" from Saf and even from his "late Tham texts." — Here again, he contradicts himself by stating that

" the loss of the last radical of the name as here is without parallel." (p. 240B)

He already forgot that he took as normal the apocopated form "rḍ" instead of rḍw (cf. above, pp. 152-153), which was an important feature of WiSLTI.

(b) Winnett's reading of the third letter from the right as "ġ" is erroneous. In Euting's copy of Eut 108 (cf. JaMAR, V, pl. 6A), the antepenult letter from the right is composed of two concentric circular curves placed vertically and open to the right; JaMAR, V, reads it as m (p. 51, in Hu 104). According to Winnett, " the third sign, consisting of two curved parallel lines, is the ordinary sign for ġ in Thamudic B (see Hu 104, 133, 431, 733, 738, Eut 94, JS 392, 600 and 601), in Thamudic C (JS 109), and in Safaitic (see WH, p. 10)." (p. 240B).

[1] Here again, Winnet is inconsistent with his basic position of restricting his argumentations to his "B" because he invokes texts from his "C" and even from Saf! Consequently, Winnett must accept as at the very least consistent with his own example the validity of other authors' recourse to all Tham texts and to Saf in their study of any Tham and Saf inscriptions.

[2] All Tham references listed by the author show a letter structurally different from the present

letter the basic form of which may be compared with the Nazis SS insignia. Even when applied to the proper letter forms, his so-called definition can lead to impossibilities; e.g., impossible names, such as "tbġ" instead of tbnn in JaT 31, and "wġ" instead of wrr in JaT 38d (cf. JaMAR, VII, pp. 109 and 112, respectively, and pl. 28).

[3] Even C. Huber had already seen that that letter form was m (cf. Journal d'un voyage en Arabie (1883-1884), Paris, 1891, pp. 533-534), and G.L. Harding does the same (cf. G.L. Harding and E. Littmann, Some Thamudic Inscriptions from the Hashemite Kingdom of Jordan, Leiden, 1952, pl. 36).

[4] Contrary to Winnett's statement, that letter form as "ġ" IS NOT TO BE FOUND in either of his Tham lists "B" and "C." That letter form,[a], placed horizontally is m in his "C" and "Tabuki;" its angular tracing,[b], also is m in his "B" = "Najdi" and "E;" [c] is the rounded form of [a] with each extremity sealed off by a short line and is also m in his "C" and "D;" finally, its angular form,[d], is also m in his "B" = "Najdi." WH 2492 (cf. WiHaI, facsimile on p. 704) illustrates the two basic forms of the letter m and ġ mentioned above: the fourth letter is [a] of m;

the ninth letter is ġ; and

the 10th letter is [b] of m.

2492

The letter form [a] is read by Winnett himself, respectively, as "m," "m," "m" and "rr" in Ḥâ'il 117, 147, 155 and 203h; cf. Berytus, 22 [1973], pp. 77, 80, 81, 88; facsimiles on pl. 8, 10, 10, 12; and photographs on pl. 9(no. 117) and 13 (no. 203h). Cf. already above, p. 156, my remark on Winnett's classification of the Ḥâ'il texts. As a consequence of his worthless statement reproduced there, the reader will never know for sure to which group the author assigns any particular text; if and when he changed his classification; and how many he finds himself unable to classify.

Ḥâ'il 117 is important: it proves that that letter form cannot be given the value of "ġ:" such a reading would yield the impossible name "hġd." Even were Winnett to consider the initial letter of the name as an incomplete "ṯ," the new name, "ṯġd," would still be as impossible as the other.

117

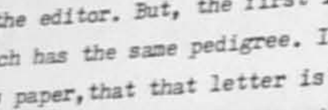
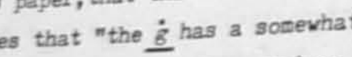
On the basis of the preceding remarks where Winnett is shown contradicting himself repeatedly without apparently being aware of it, the reader may judge the value of yet another categoric statement of his which reads as follows:

" There can be no getting away from the fact that the inscription begins with *hyg*. "O YG." " (p. 240B).

No matter how deeply Winnett feels that he is obviously right, he nevertheless is dead wrong. The text simply starts with hym, as stated in JaMAR, V, p. 51, in Hu 104. [5] WH 2194 is another good example of Winnett's epigraphical scholarship (cf. WiHaI, p. 338, and facsimile on p. 700).

" lḫwrn bn hm'ḏ bn 'ġsm wḫ.. fhlt wdt
"By Ḫaurān b. ha- Mu'ḏd b. 'Aġsam. He (grieved?) so, O 'AllĀt, [grant] love(?)."

The ḫ has a somewhat unusual form; see the form of this letter in 242 and 2983. 3202 is by the same author. - Possibly ḫ.. is to be restored as ḫ[wb]. - wdt occurs only here; cf. Ar. wadd, "love."

The letter [a] described above and open to the left is found twice as the 15th and 17th letters, and the second is read as "m" by the editor. But, the first is read as "ġ" because WH 3202 which has the same pedigree. Instead of pointing out, as in his paper, that that letter is "the ordinary sign for ġ," he declares that "the ġ has a somewhat unusual form." What he should have said is that the copyist did not reproduce the letter accurately. He also adds:" see the form ... 2983." Here follow the photostatic copies of the three letters involved:  and  in WH 2194;

in WH 242 (cf. WiHaI, p. 664); and

in WH 2983 (cf. l.c., p. 712)

The only feature common to the three letter forms is a curved double tracing; but it is immediately apparent that the basic form of the first is entirely different from that of the two others. Thus, so far, he once more contradicts the statement of his paper on the value of the first letter form and mixes up the basic form of the first and that of the two others. — Winnett's commentary suggests that "possibly ḫ.. is to be restored as ḫ[wb]" and, accordingly, his translation reads "(grieved?)." His suggestion of the verb ḫwb, which means to act unlawfully (cf. JaMAR, XIII, pp. 114-115), is less probable than, e.g., ḫwr he returned [here] (e.g., ClAS 781 in l.c., p. 77) or ḫll he encamped [here] (e.g., JaS 57b). The

width of the hatching is valueless because it is impossible to prove that the copyist did his utmost to make very sure that the width of his taching was rigorously accurate. — It should also be noted that the letter following the hatching is ġ, not "f" as in Winnett's decipherment. — An entirely different approach to the text is, however, suggested by the last word wdt which, understood as it is by Winnett as the noun for "love (?)" (cf. also p. 650), has no parallel in Tham, Saf and Ar, where the noun for "love" always is wd or wdd. The pericope wḫ..šhltwdt may be understood as wḫ..š hlt wdt and Ḫ..š - O Lat! - was in love, hlt being an exclamative interpolation between the feminine subject and its verb. An interpolation inside a Saf text is well-known; here are a few examples involving, as here, the invocation to a deity: inside a pedigree: CIS 1984 and 4889;

between a name and its tribal affiliation: CIS 4045 and Irbid 1812a;

between a word and its complement in the genitive: JaS 82b; and

between a verb and its direct complement: WH 2999.

(3) Winnett states that

" The sign after *'tm* as read as *n* by van den Branden, as / by Jamme but in Euting's copy it is clearly *ṯ*. " (p. 240B).

The first "as" of the text is a printing error for "is." — Winnett only refers to JaTV and ignores the revision of my reading published in JaMAR, V, where the sign is discarded. — It takes a cynical approach to epigraphy to declare, as Winnett does, that "in Euthing's copy it is clearly ṯ." It appears even at first sight that it represents a crack in the stone with damaged lips and the lateral cracks gave Huber the impression of an aslant ʾ.

(4) The last question dealt with by Winnett is the last word of the text, wddh. He states that

> Jamme draws attention to the fact that in six of the inscriptions (Hu 104, 126, 263, 317, 749, 769) *'tm* is followed immediately, with one exception, by *wdd* (cp. Ar. *wadda*, "to love"). The close association of *'tm* with a verb meaning "to love" confirms him in his opinion that *'tm* had a sexual connotation.
>
> The word *wdd*, which he takes as a verb, is almost certainly a noun (cp. Ar. *wadd*, *widd*, *wudd*, "lover, friend"). See no. 4, 8, 9 and 15 below.

The preceding quotation is already reproduced above, p. 148 with a series of remarks on pp. 148-149. Winnett's position is basically absurd and shows so much the type of argumentation for the sake of the cause that it is hard to believe that such argumentation is used by scholars. wddh is an asyndetic explicative phrase giving the reason why the author of the text, hym, had sex with 'm, viz. "he loved him." It is indeed impossible - and Winnett did not even try - to prove that 'm return-

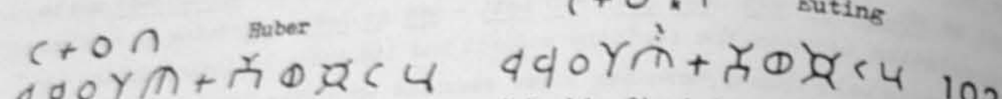
ed hym's love because he could have yielded to his advances for any other reason.

(5) JaMAR's reading (V, p. 51), which is maintained here, is as follows:

"hym ʾtm ʾm wddh
Haym has engaged in sexual relations with ʾAwm; he was in love with him."

c - Hu 114 = Eut 102 (cf. Winnett's no. 2, p. 241A).

Huber Euting



102

(1) In his first two remarks which fill almost half of his commentary (viz. almost five lines out of 11), Winnett breaks into an open door. He wants to justify the reading of both the first and seventh letter as h and m, respectively. Those corrections have already been done in 1901 by HaNEIP (cf. RS, 9 [1901], p. 324, no. 32) followed by LiZTI (p. 59), vdBrIT (p. 81) and JaTV (p. 292A).

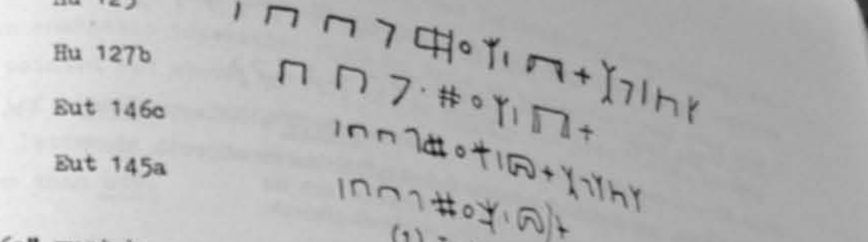
(2) Then, Winnett devotes almost six lines of his commentary arguing against vdBrIT about the word "bʿtr" where he misses the whole point (cf. below) and concludes his prose with this surprising statement:" The name recurs in Hu 114." He did not even realize that he was writing the commentary on that very text Hu 114; his "recurs" is ridiculous. — Furthermore, he also argues against vdBrIT's "hwdd" which is most certainly a slip in the author's initial transliteration of the text caused by the frequent recurrence of the root wdd in the inscriptions. The same bʿtr which occupies a line by itself, Winnett isolates it completely from the other line and considers it as "probably the name of the author of the inscription" (p. 241A). Such an interpretation should have been unacceptable even for Winnett himself because it is an hapax legomenon in his collection contradicted by eight inscriptions (cf. above, p. 149). In parallel inscriptions and according to his interpretations, that name is introduced by either l- (Hu 125, 238b, 770=775 and 801) or nm (Hu 749, Eut 94 and JsaT 368), not by "b."

(3) JaTV's interpretation of the text (p. 292A) is maintained here and reads as follows:

bʿtr 2 Biʿatar.
hrḏw ʾt(m) hʿdd 1 O Ruḏâ! Haʿadîd engaged in sexual relations with

d - Hu 125 = 127b = Eut 146c = 145a (cf. Winnett's no. 3, p. 241A).

Hu 125
Hu 127b
Eut 146c
Eut 145a



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(2) Winnett writes that " After ʾtmn Hu 125 and Eut 145a have hʿdd (an impossible root) but a reading hʿdd is supported by the occurrence of that name in Hu 114. "

Winnett's statement contains three errors. That the reading of a word "is supported" by another text is in itself irrelevant to the accuracy of the reading of the first inscription unless the two texts are duplicates or contain equivalent sections. He ignores JaMAR's (V, p. 53) remark that "the h of both khl and hʿdd is certain in Eut 146c." He also ignores a part of the epigraphical evidence. — My former reading, however, must be corrected because the 11th sign of both Hu 125 and Eut 146c, which has been considered as a double d, is awkward. Yet, the sign is clearly ḏ in Hu 127b and Eut 145a. Thus, the second half of the text may be read hʿḏlbbn, and the choice is between hʿḏ lbbn and hʿḏl bbn. The first interpretation is retained because the root lbn is more attested in Tham onomastics than the root bb.

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hkhl ʾtm/hʿḏ lbbn
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hʿḏ, cf. ʿḏ in HaLi 351.

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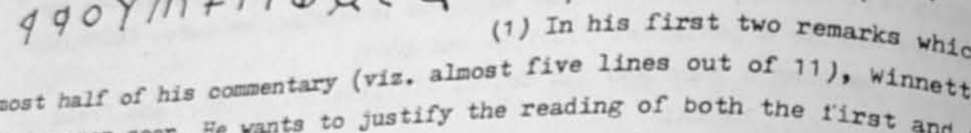
Huber Euting



102

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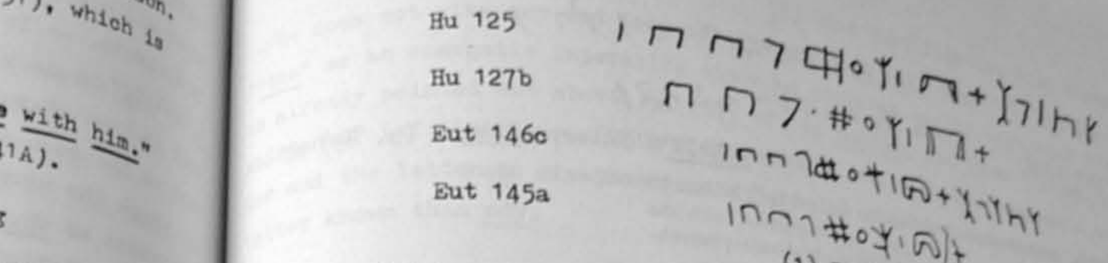

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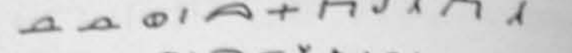
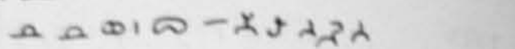
Hu 126 

But 146d 

(1) To vdBrIT's interpretation (p. 89) of the text, "O Kâhil, rends-moi parfait. Salut!," Winnett objects that

> " such a construction, with the greeting at the end, is unnatural and therefore improbable."

Why is it "unnatural"? Is the Ar leave-taking wa s-salâm "improbable"? wdd is not a greeting, but a declaration of love. And, here again, why would it be "unnatural" to express love to the deity even at the end of a text?

(2) Then, Winnett claims that

> "*wdd* can be either the name WDD as in JS 552 tham. or the noun *wadd, widd, wudd*, "friend" (see the commentary on no. 1 above)."

For this question, cf. already above, pp. 147-149. One more remark is needed. The reader notes the interesting development of Winnett's ideas for the sole sake of the cause, viz. "wdd lover" (e.g., WiReAES, p. 93B);

"wdd lover, friend" (his recent paper, p. 241A); and

"wdd friend" (his recent paper, the right column of the very same page, p. 241B).

What appears "unnatural" even at first sight is to invoke a god for an undetermined person, "a friend."

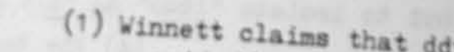
(3) As stated above, here is a case of Winnett's "unnatural" move of placing a 1968 publication chronologically after a 1974 volume. That is precisely what he does by placing JaTV after JaMAR, V.

(4) JaMAR's reading of the text, which is maintained here, is as follows:

"hkhl ʾtm/wdd O Kahil! Waddâd had engaged in sexual relations.

f - Hu 128 = But 143 (cf. Winnett's no. 5, p. 241B).

Hu

But 

(1) Winnett claims that ddy " is probably to be explained from Syr *dōd*, "paternal uncle," which occurs in Saf "

What reason does Winnett have to promote a Saf parallel after isolating his "Thamudic B" from Saf in the question of ʾtm? Furthermore, his suggestion creates an hapax legomenon in the collection.

- he does not give any and does not even mention that possibility - to consider "ʾtmn" as an energetic imperative since he could as easily have read "ʾtm ndy," as already pointed out above, p. 167; cf. Ar nadiya, 2nd form "to be, become, moistened" (cf. LaAEL, p. 3030B)? At least, I had a choice between a word divider and the letter n; the first solution was retained because the root nd is much better known than ndy.

(3) Winnett affirms that

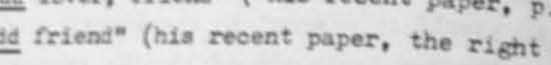
> " Euting's copy of the end of the inscription appears to be defective. "

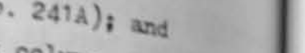
That is only half of the story: Huber's first half also is defective.

(4) JaTV's reading (p. 292A), which is maintained here, is as follows:

"hkhl ʾtm/ddy O Kahil! Dawday engaged in sexual relations.

g - Hu 129 = But 144 (cf. Winnett's no. 6, p. 241B).

Hu 

But 

(1) According to Winnett,

> " Jamme (*MAA*, V) says "the A of *Akhl* is certain." "

This is erroneous: what JaMAR says is that "the letter h of khl is certain." The difference is important because, according to JaMAR, the letter in question cannot but be the second of the divine name and, thus the third letter of the text. In Winnett's quotation, it is impossible to know whether the letter in question is the first or the third of "hkhl."

(2) JaTV's reading (p. 292B) revised on the basis of JaMAR's remark, is as follows:

"h(k)hl ʾtm hgt O (Ka)hil! Haggat has engaged in sexual relations.

h - Hu 238b = But 279b = 280 (cf. Winnett's no. 7, p. 241B).

Hu

But

(1) Winnett's commentary is devoted entirely to the reading of l.2: "

> Jamme read l.2 as /ḥrk/, taking the vertical line as a word-divider but there can be little doubt that the name should be read as ḤRKN which occurs in TIJ 504, 506 and SIJ 141, although the preceding *lam auctoris* lacks a curl at the end and hence has the same form as the final *n*. "

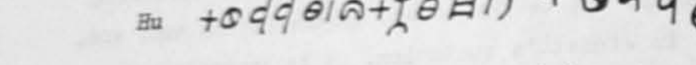
For the question of the word dividers, cf. above, pp. 154-157. Winnett's quotation

is a typical argument frequently used by Winnett. It is basically a vicious circle by repeating his reading. In fact, the main statement is based solely on the last affirmation, which, in fact accuses the two copyists of having overlooked a section of a letter (but how could both of them have done the very same mistake?) or the engraver of having twice engraved the wrong letter. Winnett's attitude is simply preposterous because the only leg he has to stand on is his own will bent on forging both copies to suit his own whim. — The information contained in his relative proposition was copied from HaAIC, p. 184. What Winnett does not say is that both HaLi 504 and Wi 141 are Saf. Here again, he is inconsistent with himself by having recourse to Saf here while refusing to do the same in the question of ʾtm. Furthermore, JaSN (p. 33, note 14) has already stated that the name of Wi 141 is brkn, not "ḫrkn." What Winnett took as the central appendix of his "ḫ" is ... the hindleg of a camel!

(2) JaTV's reading (p. 292B), which is maintained here, is as follows:

hrḏw ʾtm/ḥlṭ /ḫrk/
1 O Ruḏâ! Ḥâliṭ has engaged in sexual relations with
2 Ḫârik.

i - Hu 263 = But 309 (cf. Winnett's no.8, pp.241B-242A).

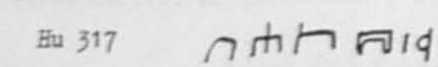
Hu  But

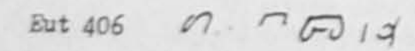
(1) He translates wdd wt as "the friend of WT," an expression which is an hapax legomenon in his collection. The fact is not brought up to the attention of his reader and, therefore, remains unjustified. Here again, he rejects the first meaning of wdd (cf.above, lastly on p. 174). The two words ʾtm and wdd are two asyndetic verbs.

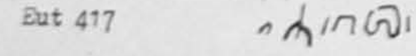
(2) JaTV's reading (p. 292B), which is maintained here, is as follows:

hrḏw ʾtm/wdd wt O Ruḏâ! Watt engaged in sexual relations [and] loved.

j - Hu 317 = But 406 = But 417 (cf. Winnett's no. 9, p. 242A).

Hu 317 

But 406 

But 417 

(1) This is the first case where, to remain faithful to his old error on "nm" (cf. above, pp. 149-150), Winnett is forced to cut the phrase into two independent sentences.

(2) Winnett does not justify his reading of "ʾtmn w(d)d" instead of the equally possible "ʾtm nw(d)d." which he does not even mention (cf. already above, p. 167).

(3) Here again, Winnett's translation of wdd as "friend" is unacceptable because the word remains undetermined. For his inconsistency in the translation of the word, cf. above, p. 174.

(4) His description of the differences between the three copies is limited to that of the 10th letter, and ends up with this: "The original reading was probably *wdd* (see no. 1, 4, 8 and 15)." (p. 242A). This reading is certain, as already given by JaTV; the word wdd is perfectly clear in But 417, regardless of But ing's question marks above the last two letters.

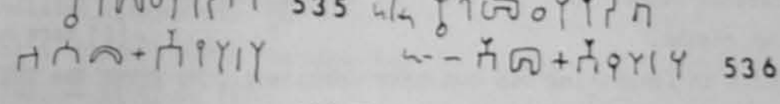
(5) Winnett eliminates the two word dividers of my translation in his quotation of my reading of the second half of the text.

(6) JaTV's reading (p. 292B), which is maintained here, is as follows:

h[r]ḏw ʾtm/wdd/mkḫb
O [Ru]ḏâ! Makḫûb engaged in sexual relations [and] loved.

Same expression as in Hu 263.

k - Hu 402 = But 535 + 536 (for Hu 402/2, cf.Winnett's no. 10, p. 242A).

Hu 

(1) It is impossible to know whether VdBrIT (pp. 199-200) considers the two lines as one text or two texts. Whether the end of l.1 is a period (e.g., here), a comma (e.g., Hu 399+398), an exclamation point (e.g., Hu 396) or has no punctuation mark which indicates that the text continues (e.g., Hu 358), he always uses the same signs before the lines, viz. "1)," "2)," etc.

(2) Winnett claims that

> " Hu 402a and 402b are separate inscriptions as shown by the difference in their pattern and by the form of the *n*. "

Here again, this is an argumentation for the sake of the cause.

(a) The argument based on "their

pattern" is a vicious circle: it is his own understanding of the two lines that created what he calls "their pattern" and, in turn, he invokes that same pattern to prove the exclusiveness of his understanding. His understanding is based on the strictly material fact that the first four letters of each line are, respectively, *bnhy* and *hnhy*. Then, following into vdBrIT's footsteps (p. 200), Winnett isolates "*nhy*" in each line and, therefore, the two lines are two different texts. This is, indeed, a very simplistic approach to Huber's copy.

[1] For the question of how many texts vdBrIT sees in Huber copy, cf. above, p. 177.

[2] Nothing can prove the existence of the divine name *nhy* in the upper line for the beginning can be read *bn hyᶜ* (as in JaTV) or *b nhyᶜ* (cf. the name *nhᶜ* in Hu 523). The first possibility was retained because the *qtl* nominal derivation is much more frequently attested than the *qtyl* formation.

[3] That the text is written in two lines should not scare Winnett off because Hu 114, 238b, 749, 770, 775 and 801 also are composed of two lines each. If those two lines are considered separately, they too show a striking difference in "their pattern."

[4] That *sk*, the author of the text, mentions the name of his father should not repel Winnett since he himself found "YSR, the mother of ʾAhil of the tribe of ᶜMN" in JsaT 249 (cf. below).

(b) The argument based on "the form of the "n" also is valueless.

[1] There is no graphic difference between But 535 and 536, and each of the two forms of *n* is found in both But copies.

[2] Here again, Winnett is very quick in forgetting his own past work; e.g., he gives the same value of *n* to two different signs in JsaT 551 (cf. above, p. 161) and also the same value of *w* to two different letter forms in Hu 215 (cf. WiReAES, p. 97 and not 76). The existence of different forms to represent the same letter is frequent; e.g., Hu 657 and HuIR 40 have each three different forms of *m*; Hu 792 (cf. above, p. 160) and 801 (cf. below) two different forms of *n* also; JsaT 296 has two different forms of *ʾ*; etc; cf. also below Hu 801, D 7 u. (3) JaTV's reading (p. 293A), which is maintained here, is as follows:

bn hyᶜ mlṭ	2	son of Hâyiᶜ [and] he flattered.
hnhy ʾṭm sk	1	O Nahî! Engaged in sexual relations Sakk,

1 - Hu 627 = JsaT 287 (cf. Winnett's no. 11, p. 242A-B).

Hu 418 𐤍𐤀𐤔𐤉𐤋+𐤊+𐤏𐤃 𐤒+𐤌𐤉𐤏𐤓𐤔𐤕 𐤊𐤃𐤉𐤀𐤉𐤌+𐤊+𐤏𐤃𐤒+𐤊𐤉𐤃𐤓𐤉𐤕

Winnett reads the text as follows (p. 242A): "*ʾlh dᶜn ʾnṭ tʾt ʾnṭ (h)w*
O god of Dᶜn, restore TʾT to health. Restore (HT)S to health."

(1) *ʾlh* is a personal divine name (e.g., Hu 418/2, 795/1, etc) and Winnett does not justify his assumption according to which "*ʾlh* is probably *ʾilāh*, "god," (vdB: *ʾIlah*) rather than *ʾAlāh* (JS)."

The normal word for "god, deity" in Ug is *ʾl*. Winnett's suggestion is all the more surprising in that in all his other texts the pericope always starts with a personal divine name - he should surely call such a fact "their pattern" (cf. above, pp. 177-178) - so much so that it is because of such a fact that he misread the third letter of Hu 104 to find another personal divine name (cf. above, p. 168). It is also because of "the pattern of the other *ʾṯm*-inscriptions" (p. 242A) that he himself rejects an interpretation of his "*dᶜn*" (cf. below). If that argument is valid in the two preceding cases, he should never have considered even the possibility of his suggestion on *ʾlh*.

Winnett had already accepted "*ʾlh*" as a noun in Ḫâʾil 203f (cf. WiReAES, p.88, facsimile on pl. 12, and photograph on pl. 13; on p. 90A, correct "302e" to "203e"), where he reads "*Ânāy šʾāk ʾlh fʿl O NHY, help me, god of salvation.*" Such an interpretation is unacceptable because his "*ʾlh tʿṭ*" should precede his "*sᶜdn*." An explicative apposition must immediately follow the word it is supposed to qualify. The possibility of an interpolation (cf. below, D 7 v) is excluded by the fact that the text may easily be translated without it. Ḫâʾil 203f may be read as follows:

hnhy sᶜd nʾl hṭᶜṭ O Nahî! Kâʾil has made Hataᶜat happy. — *sᶜd*, e.g., Hu 228a. — *nʾl*, Hu 792. — *hṭᶜṭ*, Hu 789e.

(2) Winnett states that "Huber gives the following word as *lᶜn*" (p. 242A). This is impossible: everybody knows that Huber published nothing but copies of text. Winnett simply misunderstood vdBrIt's commentary (p. 323) which gives the correct name of the author who read *lᶜn*, viz. HaNEIP (p. 349 1). Winnett got Huber's name probably from the fact that *lᶜn* is Huber's copy of the section of the text.

(3) Winnett rejects the reading of *lᶜn* because "it is improbable that a petition to curse would immediately precede a petition to restore to health" (p. 242A). Here again, such a judgment remains without any proof, and the ideas of "petition" and of "to restore to health" which are not in the

text but are his theory, prove that Winnett's statement is yet another vivious circle.

(4) Winnett states that "*It seems better to take d'n as a place-name*" (p. 242A). The sole support of this interpretation is his erroneous understanding of ʾlh.

(5) Winnett's reading includes "ʾtm" and "ʾtmn," and he translates both of them as "restore to health." He does not justify the reduplication of the same imperative under two different forms only one word apart when a single w and would have been expected and would have sufficed between the two personal names (cf. already above, pp. 165-166).

(6) On the reading of the last name, Winnett states that "*uncertain about the exact form of the middle letter*" (p. 242B). This statement is erroneous: the only uncertainty to be seen is in JaSaMA's confusion which led to a copy where the letter is dissected into two sections, the first being in stippling. Huber's copy, instead of being "uncertain," is clear and neat. Winnett further adds that "*I would suggest that it was a ṭ.*" (p. 242B). Winnett's suggestion is a gross error since his "ṭ" in "N = Najd" is an upside-down fork with square angles, while Huber's letter manifestly is "d" of his "E = Tabuki" and is easily recognizable as such in his "B = Najdi" and "C 3." Furthermore, Winnett did not invent such a suggestion; it was already made in 1901 by HaNEIP. Winnett unfortunately did not check vdBrIT's copy of Halévy's text. In fact, vdBrIT's text contains three mistakes or printing errors, viz. "bnht" instead of Halévy's "bnhṭ" in HaNEIP, p. 349 l.

(7) Winnett's reading of the text combines Huber's and JaSaMA's copies in order to suit himself, viz. he takes "dʿn" of the second, but the final ʾ of the first so as to avoid the impossible "hds." In fact, there is not a single error in Huber's copy, even the second m is present except for the upper part and a section of the first lower loop. Therefore, I retain Huber's copy, and the reading of the last three letters demands the preceding sign to be a word divider; otherwise, that name would be impossible.

(8) The text may be read as follows:

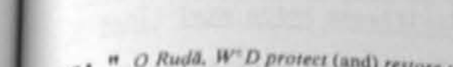
hʾlh lʿn ʾt mtʿt ʾtm/hdʾ

O ʾIlah! ʾAtt has rejected Mataʿat, [after] he had sexual relations with Haddaʾ.

My division of the first four words is identical with vdBrIT's. — lʿn, Hu 713 b/2, and Ar laʿana "abegit, et procul esse iussit" (cf. PrLAL, IV, p. 111A). — ʾtt, Hu 503. — mtʿt, cf. Ar mataʿa and matiʿa "incessit modo turpi", de femina" (cf. l.c., p. 149A). — ʾtm/hdʾ is an asyndetic explicative proposition justifying

the action related in the main phrase. — hdʾ, cf. Saf hd In CIAS 13.

m - Hu 691 a/6 = Dghty 50, 1d (cf. Winnett's no. 12, p. 242B).

Hu 

Dghty

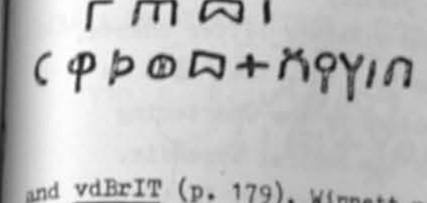
(1) Winnett's translation reads as follows: "*O Rudā, W[e] D protect* (and) *restore to health.*" — The conjunction "and" should be between brackets. — Such a screwed-up sentence is hard to believe: two imperative verbs follow their own direct complement. — Here again, he fails to explain and justify why the second imperative should be in the energetic form and the first not (cf. already above, Hu 627 on p. 180).

maintained here, is as follows:

(2) JaTV's reading (p. 291A), which is

hrḏw wʿd ʿṣm ʾtmn O Ruḏā! ʿAṣam has promised to ʾAtmân.

n - Hu 749 (cf. Winnett's no. 13, p. 242B).



(1) Winnett reads the text as follows:

"*nm il*
bnhy ʾim wdqr

By TL.
By NHY are mourning and vomiting(?)"

and vdBrIT (p. 179), Winnett reads two senestrograde lines, but JaTV reads them as a boustrophedon.

(2) As HaNEIP (10 [1902], p. 62, no.33)

(3) Winnett does not even mention the only problem of the text, viz. the antepenult letter of l.1. That letter was read as a doubtful "q" already by HaNEIP (l.c.). But, both vdBrIT (l.c.) and Winnett's transliteration considers as certain the reading of the letter as "q." The initiative of these two authors is erroneous because their reading is a correction of a letter which is not "q." H. Grimme (cf. Die Lösung des Sinaischriftsproblems, Münster, p. 65) and JaTV (p. 293A) read the letter as a doubtful "(d)." However, the most obvious interpretation of that letter is y: whether the author traced the circle first and the bar after or vice versa, the vertical bar crosses the circle; another example of that y is Hu 407 (cf. JaMAR, V, p. 76). Then, l.2 being just above the end of l.1, and the last letter to the left being dextrograde, it is quite natural to consider l.2 as dextrograde too, as JaTV has done.

(4) For his spectacular about-face on his translation of ʾtm, cf. above, p. 166. (5) To justify his interpretation of the text, Winnett refers to WiReAEB, "pp. 96-99." For the testimony presented by

WiReAES, cf. above, pp. 158 foll. (6) On his reading of the personal name of 1.2, winnett states that " TL occurs in Saf (HIn, p. 388)." Here again, winnett invokes the testimony of Saf which he has so quarely rejected in the question of ʾtm!

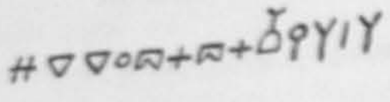
(7) The text may be read as follows:

→ lṭmn 2 Laṭīmân.

← rydw mtʾ yhnb 1 By Nahī! wudayr has engaged in sexual relations with wdyr, cf. wdr in Ḥāʾil 170 (cf. JaMAH, XIII, p. 171). — lṭmn, cf. in Saf lṭm in CIS 1153, and lṭmt, e.g., in IN 5557.

o - Hu 753 (cf. Winnett's no. 14, p. 242B).

(1) winnett reads the text as follows:



" hnhy ʾtm tmʿzz wasm
O NHY, restore Taimʿazīz to health. Wasm. "

(2) It is surprising to see Winnett simply stating that" Jamme gives to the triangular sign the value d " without the slightest remark justifying his reading of the letter as "z." Winnett cannot justify - he does not even try - such a reading on the basis of both WiSLTI's and WiReAR's letter charts. vdBrIT's letter chart (pl. 1) shows an angular diabolo as "z." But, that form is SA, not Tham. The reading of the letter as d can be explained by the shortening of the long staff of the original letter to the length of the central appendix.

(3) vdBrIT's reading of the last letter as a wasm is endorsed by Winnett. Here again, no reason is given to justify such a move. The last sign is left aside by JaTV because of the argument developed in Hu 754 (cf. below, the following number).

(4) Winnett writes this on the personal name of his translation: " The name TMʿZZ is new; perhaps it should be interpreted as "Taim (son of) ʿAzīz." " Winnett would still indulge in his old error of arbitrarily restoring "(son of)" or "(clan of)" or "(of)" or "(and)" between names, which was already denounced in BiOr, 24 (1967), p. 281A, note 9, and also in my Thamudic Studies (Washington, 1967), p. 2.

(5) JaTV's reading (p. 293A), which is maintained here, is as follows:

hnhy ʾtm tm ʿdd
O Nahī! Taym engaged in sexual relations with ʿAdīd.

p - Hu 754 (cf. JaTV, pp. 293B-294A).

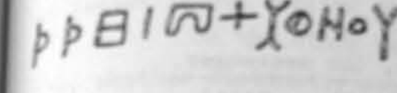
Winnett does not include the present text in his list because of an unknown reason. Yet,

JaTV's argument demonstrating the relation between Hu 753 and 754 remains valid: the second text "must be considered as repeating and completing" the first. JaTV's reading (p. 293B), which is maintained here, is as follows:

hnhy rġ (ʾt)m tm ʿdd
O Nahī! Taym acted deceitfully [and] (engaged) in sexual relations with ʿAdīd.

q - Hu 769 (cf. Winnett's no. 15, p. 242B).

(1) Winnett's reading is as follows:



" h(r)dw ʾtmn wdd
O Rudā, restore a friend (or WDD) to health. " Cf. the remarks above, pp. 147 foll. and 167.

(2) I have to apologize for JaTV's translation (p. 294A) which is contradicted by the basic principle that a verb has to have a determined object.

(3) The text may be read as follows:

h(r)ḍw ʾtmn wdd O (Ru)ḍâ! ʾAtmân was in love.

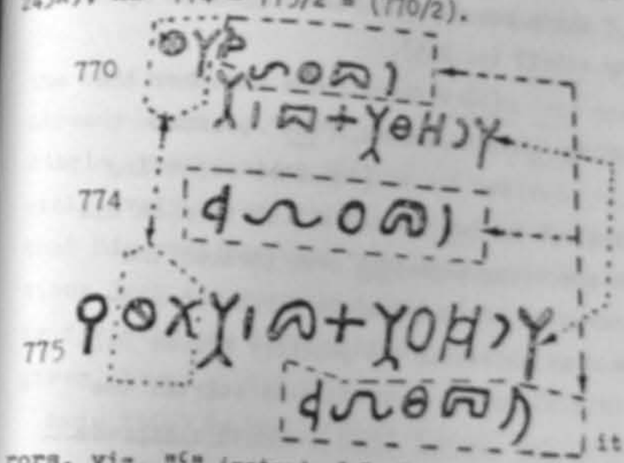
ʾtmn, cf. ʾtmt, e.g., in JsaT 577.

r and s - Hu 770 and 775 (cf. Winnett's no. 16, p. 243A); Hu 774 = 775/2 = (770/2).

770

774

775



(1) The stipplings and arrows indicate the equivalent sections of the three texts, as explained in JaTV, p. 294A.

(2) In order to justify his reading of "ʾhwy" which comes from Hu 775, Winnett states that:

" 775 is the more carefully written. "

This is contradicted by the factthat it is the copy which contains two errors, viz. "ʿ" instead of "w," and "m" instead of "l;" and Winnett himself endorses apparently without a doubt the accuracy of Hu 770 in both cases. Even the letter f of mwfd exists in Hu 770: only the extreme left curve is missing. It, thus, seems unreasonable to retain the "h" of Hu 775 rather than the h of Hu 770. Although retaining the final y of Hu 775 which is missing in Hu 770, I am not quite certain that it should be done.

(3) As already stated above (p. 149), Winnett does neither mention nor justify why he does not make any use of his theory on "lm" which he repeated even in 1981, and does not read "lm wfd" instead of "l mwfd."

(4) Winnett states this:

Ar muwaffad, "delegate." The 2nd form of Ar wafada means "to send" (cf. LaAEL, p.

2955A, and FrLAL, IV, p. 487A); thus, the passive participle means "messenger;" and it is a truism to say that messenger and delegate are not synonymous.

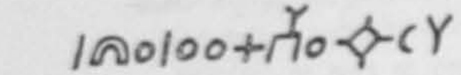
(5) In his quotation of JaTV, Winnett copies "the last three words" (my underlining), but he reproduces my translation of the last four.

(6) JaTV's reading (p. 294A), which is maintained here, is as follows:

hrḏw ʾtm nʿh wyl mwfd

O Rudā! Nāʾih engaged in sexual relations with Wāʾil, the messenger.

t - Hu 796 (cf. Winnett's no. 17, p. 243A).



(1) Winnett's bibliography fails to refer to JaMAR, IV (1973), p. 139, note 79, which contains my reading of the text.

(2) Winnett reads the text as follows:

" *hrḏ(w) ʿ(m)n ʿmn*
O Rudā, restore ʿMN to health. "

(a) He endorses the correction of the first ʿ (the 4th letter) as "(w)," which has already been suggested by HaNEIP, p. 64, no. 6, as already shown by vdPrIT (p. 235).

(b) Winnett joins together the seventh and eighth letters and reads them as a single letter, "(m)" because

" *The m-sign is composed of two small circles placed side by side, resembling the w in some South Arabian texts.* "

[1] After rejecting all recourse to Saf and "late Thamudic texts," he has invoked all of them in his commentaries, as shown repeatedly in the preceding pages; he even invoked the Hebrew adjective tamîm. Now, he has recourse to SA in a grossly erroneous manner. The common denominator of all those initiatives is the publicly avowed fallacy of the so-called imperviousness of his class "B" upon which he started the Tham section of his paper.

[2] Winnett's expression "side by side" is a fallacious play upon words: the letters could not possibly form a text were they not "side by side."

[3] The distance separating those two letters (2.1 mm), which equals that between n and ʿ, is wider than that between ʿ and n (2 mm), t and ʿ (1.8 mm), r and ḏ, ʿ and ʾ, and ʿ and m (1.6 mm in each case), and ʾ and t (1.3 mm), and is smaller only to that between the first two and the last two letters (3 mm in each case). My measurements were taken from Huber's copy.

[4] The author does not explain or justify why he selected to couple the seventh and eighth letters, the two ʿ's, rather than, e.g., the ninth and tenth letters, nʿ, which would have allowed him to read the letter ḏ.

[5] The case at hand

and that of SA w are so different from each other that it is fantasy to bring them together and irresponsibility to invoke the second to prove or illustrate the first. On the one hand, the well-known evolution of SA w (cf. a summary presentation in my chart published in W. Phillips, Qataban and Sheba, New York, 1955, p. 42) very easily explains the origin of the above-mentioned letter form; the two circles of w always are contiguous or almost contiguous; and that type of letter is not earlier than the 4th century A.D.

On the other hand, the evolution of the Tham form of m to the two ʿ's, whether distinctly separated from each other or not, is unexplainable; and MISLTI (p. 52) claims that his "B" 'would be assigned to the Seleucid period (3rd to 1st century B.C.)'."

[6] Since he endorses the correction of the first ʿ of the text to "(w)," which had already been suggested by HaNEIP in 1902 (cf. above), Winnett is illogical in not applying the same correction to one of the two ʿ's.

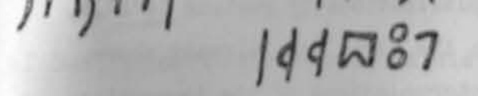
[7] Summarily, the only reason behind Winnett's choice and reading of the two ʿ's is his determination to find by hook or by crook another example of the verb ʾtm.

(c) Winnett does not justify the choice of his reading of "ʾt(m)n ʿmn" rather than that of "ʾt(m) nʿmn "(cf. already above, p. 167). Surely, that nʿmn is not yet attested in Tham is no obstacle exclusively in Saf, viz. his "hts" of Hu 627. Nor is compelling the fact that his readings show "ʾtm" being used in 11 texts and "ʾtmn" in 15 inscriptions. Such a proportion is valueless because it could easily be reversed, e.g., by dividing the texts differently from Winnett's readings. Cf. the five examples given above, p. 167.

(3) My 1973 reading is as follows:

hrḏw ʾt(w) ʿn ʿmn O Rudā! ʾAt(aw) has helped ʿAmîn.

u - Hu 801 (cf. Winnett's no. 18, p. 243A).



(1) Winnett's reading of the text is as follows: " *hnhy/ ʾtm/ ldnʾl/ l(m)dd/*

O NHY, restore LDNʾL to health. By TMDD. "

(2) On the question of the word dividers which Winnett acknowledges here, cf. above, pp. 154-157.

(3) As a proof that Hu 402 a and b "are separate inscriptions," Winnett states (p. 242A, on Hu 402b; cf. above, pp. 177-178) that it is "shown ... by the form of the n." It is incredible to see that

Winnett does not have the slightest difficulty here with the two forms of n ... in the very same line.

(4) Winnett states that "Jamme regards the / before it as an integral part of the name but it may be the *lam auctoris*."

Winnett is quite right if he considers the text as a corpse submitted to surgical dissection.

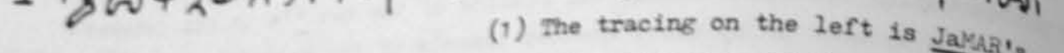
(5) JaTV's reading (p. 294A), which is maintained here, is as follows:

hnhy/ʾtm/ldnʾl/ 1 O Nahî! Ladanʾil engaged in sexual relations with
lʿmdd/ 2 Laʿamdod.

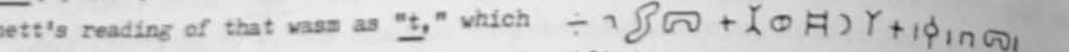
v - But 94 (cf. Winnett's no. 19, p. 243A).

Gibbon 18.X.83

94



(1) The tracing on the left is JaMAR's (V, pl. 5A), and that on the right is the photostatic copy of the upper right corner of J. Euting's Reisetagbuch, 1883, III/89. Both clearly show that the last sign, a wasm, is composed of a short horizontal bar with a dot above the line, as already clearly indicated by JaMAR, V, p. 125B, no. 17 of "geometrical wasms: " 17) ± But 63, 94." LiZTI's facsimile, pl. 2, which is reproduced by vdBrIT, pl. 12, erroneously added a dot under the horizontal bar, viz.



Winnett's reading of that wasm as "t," which he copied from vdBrIT (p. 71), is wholly unjustified. — The same tracings also show that Winnett's reading of the last letter as "r" is another error; it is l.

(2) Winnett's reading of the text is as follows:" *nm bn qnt hrdw ʾtm grt*
By Bin-QNT. O Rudl. restore GRT to health. "

(3) Winnett gives another example of his disregard of the chronological sequence of some publications by quoting JaMAR, V, before JaTV ... as if the second was posterior to the first (cf. already above, p. 167).

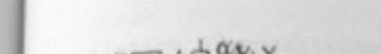
(4) For interpolations of the invocation to a deity inside a Saf text, cf. above, p. 171, on WH 2194. In the present inscription, hrdw, is inserted between the subject of a verb and the verb itself. Winnett's translation is a consequence of his erroneous theory on "nm" (cf. above, pp. 149-150) and in the process he invents a new Tham nominal derivation, viz. "bnqtl" in his "bn qnt."

(5) JaMAR's reading, which is maintained

here, is as follows:

"nm bn qnt hrdw ʾtm ġl wasm
Nawm, son of Qânit, - O Ruqâ! - has engaged in sexual relations with
Ḏuil. wasm."

w - JsaT 335 (cf. Winnett's no. 20, p. 245A-B).



(1) Winnett copied his transliteration from vdBrIT (p. 339) who had copied his including his name "dbʿd." The great novelty of Winnett's commentary is to suggest about his "dbʿd" that "Perhaps it means "a distant one." ," thus accepting once more (cf. already "wdd" in Hu 126) that the complement of a verb would be an undetermined person. Besides, his "dbʿd" would mean "he [who was] distant."

(2) The top of the eighth letter is covered with hatching, which means that it is incomplete. Therefore, Winnett's and his predecessors's reading of the letter as "n" is excluded because the hatching indicates that the top of the letter is destroyed. Since the hatching is placed in such a way that there is not enough room on the right to restore the circle of the letter y, there, thus, remains only the reading of "(l)," as already indicated in JaTV.

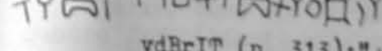
(3) Contrary to his ostracism of Saf, Winnet refers to it in his commentary on his "dbʿd."

(4) JaTV's reading (p. 294A), which is maintained here, is as follows:

hnhy (ʾ)tm (l)d bʿd
O Nahî! (La)wd (engaged in) sexual relations with Baʿad.

JaTV's text contains several printing errors, viz. all parentheses were omitted, and ʿ of the transliteration became "ʾ" in the translation.

x - JsaT 368 (cf. Winnett's no. 21, p. 243B).



(1) Winnett copied his decipherment from vdBrIT (p. 313):" *hrd(w) (ʾ)tmn tʿly nm yz*
O Rudl. restore TʿLY to health. By YZ. "

(2) Here again, in spite of his ostracism of Saf, Winnett refers to it in his commentary on his "yz."

(3) For the question of the word dividers, cf. above, pp. 154-157.

(4) JaTV's reading (p. 294B), which is maintained here, is as follows:

hrḍ(w) (ʾ)tm/tʿly/myz
O Ruḍ(ā)! Taʿalay (en)gaged in sexual relations with Miyazz.

y - JsaT 409 (cf. Winnett's no. 22, p. 243B).

𐩣𐩲𐩧𐩫𐩥𐩱𐩧𐩣𐩲𐩨𐩧𐩫𐩱𐩥𐩧𐩫𐩣𐩲𐩧𐩫𐩱𐩥𐩧𐩫𐩣𐩲

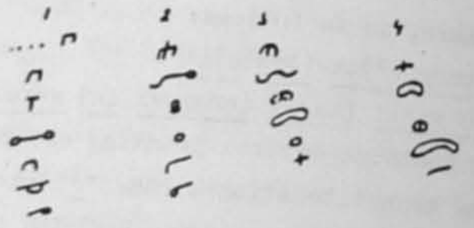
(1) Winnett's reading is as follows:

" h²lh dhwn ²tm ysr ²m ²hl d²t ²(l) ᶜmn
O god of DHWN, restore to health YSR, the
mother of ²Ahil of the tribe of ᶜMN."

(2) Winnett devotes more than half of his commentary, viz. 15 and 1/2 lines out of a total of 24 and 1/2, to prove his thesis, viz. the divine epitheton dhwn, which immediately follows ²lh, is *" More probably it was the name of a region."* The basis of his interpretation, viz. ²lh is a noun instead of a proper name, is erroneous; cf. above, p. 179 on Hu 627. - Hypnothized by JaSaMA's etymology (p. 584) referring dhwn to the root dhn, either 1st or 3rd form (from which they translate dhwn as "indulgent"), Winnett invokes the testimony of Ph 271z, which shows "dhn" instead of dhwn. It has become a well-known fact that the sole reliability that may be granted to Philby's copies is their unreliability. The last example in date illustrating the accuracy of such a fact is given in JaMAR, XIV, p. 8, which is reproduced here for the benefit of the reader.

(my tracing of JaT 83a-d from photographs)

PHILBY 218 (a)



(Philby's drawing of the same texts from the boulder itself)

The main fact to be underlined here is that Philby's copy adds one inexistent letter at the beginning of text a; and omits one letter in text b and two in text d. It is, thus, unbelievable that any scholar would be so foolish as to still rely on Philby's copies to prove a point unless he is so blinded by his own preconcieved ideas that he is ready and willing to consider as a man a dog masquerading with a hat on its head and a necktie around the neck.

For dhwn, cf. Ar dahâ (o) = dahiya = dahá (i) "to possess cunning, i.e., intelligence, sagacity" (cf. LaAEL, p. 927B). Such a meaning is eminently fitting a deity.

(3) On his "ysr," Winnett refers to HaAIC, p. "170" which must be corrected to "670." None of the references given by HaAIC is Tham, and two of them are Saf. Here again, it is against his ostracism of Saf. Furthermore, ysr is a masculine name, not a feminine one.

(4) The reader is puzzled by Winnett's initiative of vocalizing his "²hl" as "²Ahil." He did not vocalize a personal name in all preceding texts! Why does it here? Who knows? Why does he retain the vocalization of an Ar word which means "a domestic beast or bird" (cf. LaAEL, pp. 121C-122A)? Who knows? He also refers his "²hl" to HaAIC, p. 82, where reference is made to "Hu 516, 8" = Hu 734. In this text, ²hl is a present participle (cf. JaMAR, VII, p. 189A). But, the personal name ²hl is attested in Hu 5 (cf. JaMAR, IV, p. 131, note 45).

(5) The crux interpretum is the eighth letter from the end. It has been in the past erroneously read "ḏ;" the two vertical bars being clearly distinct, 𐩤 = 𐩣, the letter can only be ḏ. Even Winnett should agree with such an identification because of his WiSLTI's chart on "Ḏ." But, should he prefer the same chart on "B," the references given there under that letter form would provide him with several possibilities to choose from. According to that chart, that letter form is found in Hu 125, 448, 450, 464 and 627. In the present paper, the letter of Hu 125, his no. 3 (cf. above, p. 173), is understood by him as "dd;" in Hu 627, his no. 11 (cf. above, p. 179), the letter becomes "(ṯ);" and WiSLTI (p. 32) gives the value of "d" to the letter of Hu 464. I do not know what Winnett does with the two remaining texts, viz. Hu 448 and 450. The preceding reading of the letter as ḏ demands the reinterpretation of the balance of the text. Therefore, there is no need to amend the fourth letter from the end to "(l)," a correction for which Winnett thanks "M.C.A. Macdonald, (private communication)," although he himself quotes JaTV's text where that correction stares anyone, including the blind, in the face.

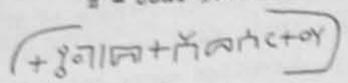
(6) The text may be read as follows:

h²lh dhwn ²tm ysr ²m²hl ḏ² t²y ᶜmn

O ²Ilah, the Intelligent! Yasirr engaged in sexual relations with ²Umm²ahal; he was gleaming, having preceded ᶜÂmin.

ḏ², cf. Ar ḍâ²a (o) "to shine, glitter, gleam; give light; irradiate" (cf. SaAAL, p. 485B). — ty², cf. Ar ta²á (i) "praecessit et praevertit" (cf. FrLAL, I, p. 182B).

z - JsaT 576 (cf. Winnett's no. 23, pp. 243B-244A).



(1) Winnett's reading of the text is as follows:" *h'trsm 'lmm l'gt*
O 'Atarsamm, restore Li'agt to health." (p. 243B).

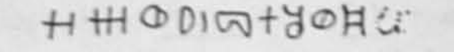
(2) Winnett's understanding of the name l'tt as "l + 'tt" (p. 243B) and, therefore,his vocalization as "Li'atat" are erroneous. This is the second and last personal name vocalized by Winnett in the whole collection, the first being his "'hl" of JsaT 409 (cf. above, p.189). Winnett invents the Tham inexistent form "'tt." What exists in Tham is the form 'tr. Furthermore, were Winnett's etymology given any consideration, l would be "il" (l = 'l), not the preposition. Summarily, it is regrettable that Winnett did not listen to HaAIC (p. 516), who correctly refer l'tt to the root l't.

(3) JaTV's reading, which is maintained here, is as follows:

"O 'Atarsamm! La'itat engaged in sexual relations."

'trsm: : the form 'tr was almost next door to l'tt and Winnett did not even see it!

aa - Dghty 47 (4) (cf. Winnett's nos. 24, p. 244A).



(1) Winnett's reading is as follows:

" *hrdw 'lmm gwṯ* Wasm?
O Rudâ, restore GWṮ(?) to health. Wasm? "

(2) Winnett considers the last sign as a "Wasm?" and reads the three preceding letters as "gwṯ" with the note that "The reading of the personal name is uncertain." The actual meaning of this remark is not clear. Since the commentary does not mention the question of the "Wasm?" at all, and there is no parentheses or question mark to indicate any doubtful letter in his transliteration of the name, the uncertainty seems to be only on the number of letters composing the name, viz. three or four? The last letter is of the same type as that of Hu 753 (cf. above, p. 182) which Winnett excluded as a wasm (an easy solution to get rid of an embarassing letter, so it seems) without any justification. He does the same here: he does not explain the reason of his opinion. It seems, however, rather easy to see that he did not know what to do with

the letter immediately following the three letters of his "gwṯ." The exclusion of a letter at the end of a text without any justification is unacceptable.Furthermore, both the last and the penult letters have in common that the horizontal stroke does not go beyond the right vertical stroke; this remark does not corroborate JaTV's assumption that there were two letters written together. The two letters belong to the same type of ṯ with two or three vertical strokes.

gwṯ, an unknown name. " If the remark implies that the name is rejected because it is "an unknown name," it is erroneous. The Ar root ǧaḏaba exists, and in the Ar word referred to by vdBrIT the "d" is a printing error for "ḏ." Furthermore, vdBrIT himself declares that"le b est douteux." If Winnett's above-quoted remark does not have any hidden implication, it simply means what it says. Why, then, does he not mention also that his reading "gwṯ" is "an unknown name," beside being an unknown root.

(3) Winnett writes that vdBrIT " gives it as gwṯ," cf. above, p. 167.

(4) For the gratuitous reading of "'tmn wṯṯ, cf. wṯ in Hali 297.

(5) The text may be read as follows:

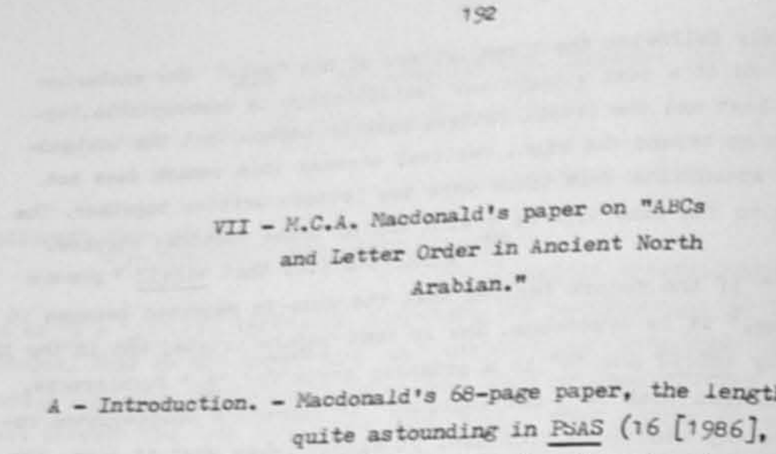
hrḍw ('l)tm ng wṯṯ
O Ruḍâ! Nawg engaged in sexual relations with wâṯiṯ.

The preceding pages contain an almost countless number of irregularities of all kind in Winnett's method which may be summarized as based solely on arguments for the sake of the cause regardless of self-contradiction,about-face, good common sense and any writing, not even his own.

VII - M.C.A. Macdonald's paper on "ABCs and Letter Order in Ancient North Arabian."

A - Introduction. - Macdonald's 68-page paper, the length of which is quite astounding in PSAS (16 [1986], pp. 101-168), may be singled out as one literary production bearing the stamp of G.L. Harding's legacy and P.V. Winett's inspiration. It would take a whole volume, if compared with my rebuttal of Winnett's last paper of less than six pages (cf. above pp. 124-191) to discuss and straighten out the mass of its slanting, biased and plain erroneous statements and/or arguments. I am unwilling to sacrifice the time that the thorough study of such a paper would inevitably demand. However, as a typical sample of Macdonald's argumenting method, his very first argumentation is taken up below. The choice of this section shows that I did not purposely select a weak or the weakest part of the paper. The reader may then judge by himself the actual quality of that section and draw the conclusion of his choice.

B - Macdonald's documentation. - Here follows his documentation. The



" l ʕ ᶜ b n ẓ w g ṯ d r k s² f m h ġ q ḍ ṭ ( ) s' ḫ ḏ ' ḥ t z y

Fig. 1 : Safaitic alphabet (MaSA)"

(p. 102, bottom)

(p.102, center)

" I would read

l ʕ ᶜ b n ẓ w g ṯ d r k s² f m h ġ q ḍ ṭ ( ) s' ḫ ḏ ' ḥ t z y

At first sight this appears to begin in the standard Safaitic manner, with the 1ām auctoris + Name + bn + Name. However, while such a reading produces two possible, if somewhat bizarre, names (10), it is clear that what follows is unintelligible if approached in this way. It will also be obvious that the inscription consists of one example (and one only) of each letter of the Safaitic alphabet. It seems to me probable that the author originally set out to carve an inscription beginning (11):

l ʕᶜb bn ẓᶜn....

and that, when he noticed that he had omitted one of the bs and had left himself no space in which to insert it (12), he abandoned his original intention and decided to write out the alphabet instead. If, as I have suggested, the second name was ẓᶜn, he showed considerable ingenuity in converting his second ᶜ into a w and the second n into a g (13)."

(p. 132)

9.." 10. A name ʕᶜ is not impossible, though as far as I know it is not attested. ʕw has been found once (c.1853). ʕwg would be less likely since the root is unknown in Arabic.

10. 11. If this explanation of the initial sequence of letters is correct, these would seem to be the most probable names, though, needless to say, there are other possibilities.

11. 12. A similar case of haplography affecting this name can be found in WH 1716. That the name is intended to be ʕᶜb is confirmed by a comparison with WH 1707 where the same genealogy is carried back one generation further.

12. 13. The lozenge form of the g seems to favour such an explanation. The bottom left-hand side of the lozenge could easily have stood by itself as an n. However, it cannot, of course, be assumed that the name was actually ẓᶜn, or that, even if it was intended to be, that the author had actually written the -ᶜn before changing his intention. Certainly the shape of the w is quite different from that of the ᶜ earlier in the text. The analysis below is not affected by where exactly the author abandoned the name."

(p. 103, center)

13.... "MaSA: l ʕ ᶜ b n ẓ w g ṯ d r k s² f m h ġ q ḍ ṭ s' ḫ ḏ ' ḥ t z y"

14.. (pp. 101-102) "where it turns, at the junction between lines 1 and 2, there is a sign like a f (though its top curves back towards the stem unlike the example earlier in the text). It is possible, of course, that it is a mistaken repetition of f, though in view of the arrangement of the letters (see below) I think this is unlikely. I would tentatively suggest that without this sign the two lines would appear unconnected and that, without the flow of meaning and conventional phraseology which in a normal inscription would ensure that the reader was drawn from one line into the next, there would be nothing to link them. It is possible that, realizing this after the text was finished, the author added this sign as a way of showing that the two lines belonged together."

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letters and numbers are added to his facsimile and text, respectively, in order to make the references to each section of them as easy as possible.

C - General remarks.

1 - The photograph of the text at the bottom of p. 168 is poor and does very unfortunately not show all letters distinctly. Therefore, both the systematic checking of every letter of Macdonald's facsimile and the complete reading of the text without which no complete study and no translation can be attempted, are regretfully not possible.

2 - Macdonald's facsimile does NOT faithfully represent several of the letters of original which appear clearly enough on the photograph.

3 - According to the editor, the text is "a Safaitic alphabet (referred to as MaSA)" (p. 101). Such a siglum is a reminder of another one, "TTI" (cf. JaMAR, IX, pp. 3-6), which was born in similar circumstances and "will forever remain the silent witness of a preconcieved idea."

4 - Without saying a word about it, Macdonald rids himself of his previous habit of transcribing the sibilants š and s; he now endorses Beeston's latest (cf. Orientalia, 48 [1979], p. 263, and JaMAR, XIV, p. 70) transcription system. The reader wonders whether his shifting was an admission fee before being "invited to serve on the Committee" (cf. PSAS, 15 [1985], p. 34) or a good-will gesture, a pacifier or a mollifier devised to finally curry favor with some diehard opposition to the invitation.

D - Discussion of Macdonald's thesis.

1 - No. 1. - The initial statement is subjective and somewhat childish. Many texts do not start with a pedigree; e.g., JaS 44, 57c , 58c, etc, and the presence of bn near the beginning

of a text does nor prove at all that it is the noun bn son of any more than bn at the end shows a text to be incomplete if not followed by a hatching (e.g., JaS 57c).

2 - Nos. 2 and 9. - The reading of the beginning based on his initial reaction would yield "two possible, if somewhat bizarre, names." Such a statement aims at creating a climate of distrust in the reader's mind; it is just what Macdonald wanted to achieve whether right or wrong.

a - The initial name would be "ġ^{c}" which no.9 declares to be "not impossible" as if "possible" and "not impossible" were exactly interchangeable. The reader immediately notices that the stigma of "somewhat bizarre" is not repeated in no.9, but the harm is done and remains because the reader may not go to p. 132 to read no. 9. What is "not impossible" or "somewhat bizarre" in a name, such as ġ^{c} Ṣaw^{c} (cf. Ar ġâ^{c}a and the noun ġâ^{c})? It is attested in Tham, e.g., Ḫâ^{3}il 139a (cf. JaMAR, XIII, p. 169); cf. also Tham ġw^{c} in Hu 228b (cf. JaMAR, V, pp. 62-63). If Macdonald states that "as far as I know it [= ġ^{c}] is not attested" in Saf, he has nobody to blame but himself for his ignorance is but the consequence of his blindly following into Winnett's footsteps, viz. his mistreatment of Wi 1716 (cf. below, no.11); the name ġ^{c} does exist in WH 1716. Even were ġ^{c} unattested before, which is not the case, Macdonald would still have no reason to justify his "not impossible" and/or his "somewhat bizarre." Depending on the context, the initial name could also be ġnbn Ṣinbân; cf. Ar ġinâb "a sauce made of mustard" (cf. LaAEL, p. 1730c).

b - The same no.9 indicates that the second name is either "ġw" or "ġwg." Since he himself has to admit that ġw is attested in CIS 1853, it is dishonest to state, as he does, that the name is one of the two "somewhat bizarre, names" (cf. his no.2). — Of course, nobody in his right mind would dream to support a name without any connection with a Semitic root. But, Macdonald's remark on "ġwg" to the effect that "the root is unknown in Arabic," shows how ignorant he is of Saf onomastics. Indeed, "ġwg" would be a qwtl derivation from the root ġgg; cf. Ar ġajja "clamavit opem petens in praelio" (cf. FrLAL, III, p. 86A); cf. already ġg in WH 471. Besides, the third letter of the name, letter B (cf. below, no. 3 b) is ʿ, not "g." Thus, the name would be ġw^{c}; cf., e.g., ṯ^{c} in CIS 3121.

c - WH 471 (cf. WiHaI, p. 119, and facsimile on p.

670) is read by the editor as follows:

"471 l‡'nl(?) (p184)
"By ‡a'nil(?)."
‡'N'L occurs in C 2328."

Winnett's decipherment is here again vitiated by his obstinate desire of finding a well-known name or something similar. His reading of the third letter as "c" is disproved by the size of the letter, and that of the last letter as "l" is erroneous. It remains impossible to know whether his question mark after the last letter of both his transliteration and translation (cf. also p. 590B) refers to the whole name or to the last letter only. — I cannot figure out how HaAIC (p. 426) could refer the name "cqb" to this text. — The text may be read as follows:

l‡g nb By ‡agg. He has come [here] time after time.

‡g, cf. trg in WH 460; for the form of g, cf. below, nos.4 etc, c. — nb, e.g., CIAS 1057 (cf. JaMAR, XIII, p. 96).

3 - No. 3. - Macdonald's statement not only is valueless for his thesis but also shortsightedly breaks into an open door. How can anyone in his right mind even think of a pedigree in a text lacking the necessary hinge bn son of connecting the names together? If, however, his statement implies that the whole text "is unintelligible," viz. that it cannot yield any sense unless understood, as his thesis has it, as some kind of "ABC," he should have proven it instead of merely stating his thesis again in such obvious a vicious circle; and the proof is still wanting.

4 - Nos. 4, 12, 13 and 14. - The statement of no.4 being another vicious circle, shows how tremendously blinding the power of his preconcieved thesis really is. It only proves that such was his preconcieved idea which bends the letters to become the reality of his dream.

a - Letter A looks like an elongated hairpin with the right end slightly open. For Macdonald, it is "c" in all certainty because ... he needs an c. Yet, the value of the double tracing of the letter cannot be found in the gloomy obscurity of a preconcieved thesis ... independently from the text itself. The same feature of double tracing is also attested in letter J with apparently a decorative value only. Thus, the letter A cannot be read as "c;" it is a simple n.

b - Letter B is too small in size, somewhat larger than the preceding w, to be read as "g;" it is c. The measurements of the two

letters w and c on the photograph are, respectively, 2 x 3.8 and 2.3 x 4.2 mm. Furthermore, the first statement of no.12 speaks of "the lozenge form of g." Since the letter is an almost rectangular quadrilateral with opposite sides parallel and unequal, it is a parallelogram. That letter form with slightly rounded angles is attested, e.g., in WH 471 (cf. above, p. 196).

c - Even in Macdonald's facsimile, the diacritical top of letter C is in stippling. But, the part in stippling cannot be seen on the photograph; the letter is y, not "t." The two y's of the text have in common that the diacritical triangle is appended to the bottom of the shaft.

d - The tops of letters D and E also are in stippling in his facsimile; they are missing on the photograph.

e - Letter P as represented in his facsimile is not on the photograph; the whole letter is difficult to read but might look like(?) a h with the lines converging to the top.

f - The upper 2/3 of the letter G of the facsimile cannot be checked on the photograph.

g - The letter H is misrepresented in his facsimile. His mistreatment of the letter is found in less than ten lines of no.14.

(1) As Winnett's staunch follower, the first idea that popped into his mind was this: "it is possible, of course, that it is a mistaken repetion of f" (cf. no.14). What a pity that the engraver did not have Macdonald to teach him his own native language and his stone carving! But, Macdonald did not really mean what he wrote. It is immediately obvious that such a possibility in fact is impossible because it would destroy the very heart of his major statement which has to be true at all cost, viz. "one example (and one only) of each letter." Consequently, although the location of the letter is indicated in his transliteration of p. 102 by "( )," even these two empty parentheses disappear in his next transliteration of no. 13. Thus, gone with the wind and swept under the rug is that miserable letter which so callously interfered with his great dream. It should none the less be repeated most emphatically that he contradicts himself openly: the location of the letter appears on both pp. 101 and 102, but is eliminated on p. 103.

(2) Since the letter is there, Macdonald has no choice but to prove that it is not a letter after all and, therefore, that it has no right to be among others in no.13. His genial explanation is that "the author added this sign as a way of showing that the two lines belonged together."

In other words, the letter "f" is nothing more that the siglum standing for "f[ollow me]" as the signpost between two separated ruts. In fact, Macdonald's remark is another proof of his deep ignorance of Saf epigraphy and shows his unwillingness to find out the facts, unwilling, as he is, to face the obvious testimony of the texts in order to be free of making his stupid remark. There are, indeed, many Saf texts with no connection between their lines. Here are a few examples: the three lines of both WH 607 and 609, and

the two lines of wi 611 (all three texts on the same p. 86 of WisIJ)

are not connected with each other;

the end of the first section and the beginning of the second of CIS

4262 are 13 (thirteen) centimeters apart;

the two lines of CIS 1185 and

the two sections of CIS 2732 are not joined together by anything;

etc.

h - A short horizontal line closes the upper extremity of letter I and is simply disregarded in Macdonald's facsimile; the letter should be g.

5 - Nos. 5 and 10. - The statement of no.5 is a useless waste of time as if he was flailing the air. What he must do is to understand what the writer engraved and nothing else. Besides, his no.10 destroys his own concoction by saying that "there are other possibilities" without obviously enough mentioning any of them or even giving any reason for his choice. Cf. also below, no.8.

6 - Nos.6 and 11.- a -There we go again with another assumption that the writer made a mistake by forgetting "one of the bs." According to Macdonald, the writer "had left himself no space in which to insert it," viz. the famous missing b. Really, Macdonald displays a complete lack of imagination. It is he who will extol in no.12 the "considerable ingenuity" of the engraver. If so, how could he write that the very same worker forgot one letter in the pedigree composed of his own name and his father's and, on top of it, that he "had left himself no space in which to insert" the famous missing b? Of course, he could have added that b between the existing b and the following n if he had to. These two letters are separated from each other by 1 mm on the photograph. Did he not engrave the existing b almost against the preceding n? But, if he had that kind of "considerable ingenuity," he would have attained the same result in a much easier way by simply adding to each extremeity of the existing n a slight curve parallel

to that of the existing b so as to transform the n into b and also by engraving the new n between the new b and I. There is a blank of 4.3 mm to the left of that I, viz. there is plenty of space for the narrowest of all letters, n.

b - No.11 is another outstanding example of a sheepish, irrational attitude. There is not a single iota of truth in his statements. Here follows WiHaI's documentation, respectively, pp. 274, 275 and 691 (for the two facsimiles).

1707 lʕ'b bn d'b bn n'mn bn kn
"By ʕa'b b. Di'b b. Nu'mān b. Kaun."

1716 is by the same author

1716 lʕ'b [b](n) d'b bn n'mn bn ...
"By ʕa'b (b.) Di'b b. Nu'mān b. ..."

1707 is by the same author.

The reader of Macdonald's statements cannot but be convinced that Macdonald discovered the information on WH 1707 and 1716 himself. Nothing could be farther from the truth. Instead, he sheepishly endorses WiHaI' opinion without acknowledging his dependence. Winnett's opinion is based exclusively on his well-known trigger-happy attitude, which I always rejected as very offensive to the native writers, of correcting the texts to suit his own ideas at the expense of the respect due to each writer. After all, none of those ancient writers ever wrote to please Winnett or Macdonald or anyone else. Note also that HaAIC (p. 372) presumably endorsed Winnett's opinion because he states that the name gʿb is found in eight WH texts, and WiHaI's p. 588B also lists eight references. Contrary to Winnett, Harding and Macdonald, there is no haplography of one b in WH 1716; the name of the author is gʿ Sawʿ (cf. already above, p. 195); cf. above, p. 22, Ja 3171 i/1.

WH 1716 may be read as follows: lgʿ bn dʾb bn nʿmn bn [kn]

By Sawʿ, son of Diʾb, son of Nuʿmān, son of [Kawn].

The epigraphical evidence given by the beginning of WH 1716 proves that Macdonald's opinion is NOT, as opposed to his statement, "confirmed by a comparison with WH 1707" ... for the identity of pedigree does not, and cannot, exclude the names of two brothers as they are written in the facsimile. Macdonald is also dead wrong when he states that the pedigree of WH 1707 "is carried back one generation further." This statement shamelessly eliminates the last bn of WH 1716 which, obviously enough, was introducing the name of the ancestor mentioned

last. This name was apparently destroyed but its presence is clearly indicated by the hatching. To ignore those two epigraphical facts, as Macdonald does, although they are staring him in the face; to be ever ready, as he is, to accuse an ancient writer of engraving errors without the slightest proof, such attitudes are the very denial of what epigraphy stands for. The authors who indulge in such grossly erroneous gimmicks and practices to attain whatever goal they may have in mind and their friends who smugly endorse and applaude to such behavior banish themselves from the community of the scholars who rightly believe in the objectivity of the texts and scientific honesty.

7 - No. 7. - This statement calls for the Latin axiom "Quos vult perdere Iupiter dementat." It is, indeed, insane to be so blinded as to decide himself that the writer changed his mind and that he chose to do exactly what he himself expected him to do.

8 - Nos. 8 and 12.

a - Here again, Macdonald does apparently not understand the words he uses. The initial statement of no.8 is "If, as I have suggested, the second name was z̧ᶜn." But, all that he says in no.5 is "it seems to me probable that the author originally set out ... z̧ᶜn." And to make things even worse, no.8 refers to no.12, where he flatly contradicts himself saying that "it cannot, of course, be assumed ... z̧ᶜn, or ... the -ᶜn." But, who is going to compare the three texts? The ordinary reader will most probably not check no. 8 against no.5 and almost certainly not no.8 against the footnote of no.12.

b - His statement of the two corrections is ridiculous. If Macdonald's assessment of the writer's "considerable ingenuity" is correct, he should have explained why the worker did not use his remarkable resourcefulness and clever workmanship to engrave his own name and his father's correctly in the first place and , I F he made some mistakes - but that point has to be proven - , to correct those two names instead of acknowledging himself beaten. It would have been so easy, as stated above in no.6 (pp. 198-199), to correct the two names ... provided, of course, that they were miscopied. The thought mentioned above must be strongly emphasized: a human being knowing how to write could not even write his own name and his father's without errors! How preposterous it is and especially from an author who praises the worker for his "considerable ingenuity"! But, contrary to Macdonald's assessment, it most certainly does not require any ingenuity or special skill to correct "ᶜ" to "w" and "n" to "g" ... it suffices to add a stroke inside "ᶜ" to make a "w," and

three short strokes outside the "n" to make the parallelogram of "g." Furthermore, the initial statement of no.12 is an empty babbling: there is no way the form of his "g" can "favour" any of his concoctions for the simple reason that there is no way he can prove that the letters involved were not engraved in the first place as they now stand on the stone.

E - Summary of Macdonald's thesis.

Since an "ABC" is by definition an alphabet, Macdonald's thesis implies a contradiction in the terms because, seen from his point of view, the present text presents o n l y a series of letters tossed together in an apparently disordered fashion. This erroneous misrepresentation of what an alphabet is, Macdonald did not invent it; he copied it from J. Ryckmans whose work is characterized a "deliberate misrepresentations and/or distortions of the written word" (cf. JaMAR, XIV, p. 175; for examples, cf. pp. 175-202, and passim pp. 202-268).

His thesis actually is an unsound scenario imposed by the preconcieved thesis and composed of the following elements:

1 - a speciously distorted and biased analysis of the letter forms written for the sake of the cause;

2 - the gratuitous statement on the misfit in the short pedigree by a man of so-called "considerable ingenuity,"

3 - the gratuitous statement on the correction by the engraver of at least two letters in the pedigree;

4 - the gratuitous statement on the engraver's change of mind; and

5 - the misreading of at least three letters.

These are the basic ingredients of Macdonald's dish; the condiments, spices and binder are provided by his argumenting method and all his biased, tricky, and erroneous gimmicks.

F - The purpose of the text.

On the one hand, a Saf person certainly could wish, if he felt up to it, to prove not only to himself but also to all passing-by travelers that he was quite capable of composing a text where he would use as many letters as possible. On the other hand, what could have been the purpose of a so-called "ABC," which is not an "ABC," scratched almost near the ground, "some 15 cms. from the present ground level at its lower point" (p. 101) and composed of letters ranging in height from 2.2 to 4.6 cm according to the scale added to the

facsimile of the text with the shortest n, w and c measuring, respectively, 0.85, 1 and 1.15 cm.

Macdonald presents his ideas on the whole problem as follows:

1. " The careful arrangement of the signs in MaSA implies long familiarity with the
2. letter-forms, rather than the scribblings of a beginner, and its proximity to the
3. "exercises" is thus inevitably suggestive, even though we cannot be sure of the exact
4. chronological relationship between them (27). It is, therefore, quite possible that, even if it was not originally intended as an aid to instruction, it was later put to pedagogic use."

1 - No. 1.

a - This statement is manifestly biased: the so-called "careful arrangement" is a myth dreamed by Macdonald because he needs it. In his description of the letter forms (p. 105), the reader can find an excellent example of a description best fitting his thesis. Two examples suffice here.

(1)" r and k are shallow curves with two hooks and one hook respectively."

The so-called "hook" in k is ... longer than the staff: what a "hook"! The description compares together the essential section of r with the appendix of k; the structure of the two letters is entirely different in each case.

(2)" ḥ, ḫ, ṯ, ẓ are varieties of cross." The essential characteristic of ḥ is that the two independent appendices do not cross the central stroke; otherwise, it would be h. The characteristic of ẓ and also of ḫ is that the transversal bar does not go across and beyond the central staff. In both h and ṯ the transversal bar goes across and beyond the central staff. Were the animals categorized in the same manner as Macdonald presents these letters, ostrich and giraffe would be in the same bag with the label "king-size necks."

"The careful arrangement" on the basis of the letter forms would have yielded the letter groups as follows: n and l; d and q; b and r; s and k; y and ṯ + h, ḥ and ḫ. There is no ṯ in the text (cf. above, p. 197 c).

The biased presentation of this no.1 is aimed at surreptitiously instill into the reader's mind the idea that the text is some kind of model well worth imitating.

b - The implication of a "long familiarity with the letter forms" is another inflated balloon. What kind of revelation did Macdonald receive on that so-called "long familiarity? But, such a remark too prepa-

res the reader to what Macdonald has to say.

c - The truth of the matter is that the general carelessness of the engraver suggests a poor writer, viz. someone who did not write on stone frequently.

2 - No. 2.-What are those so-called "exercises"? They are already mentioned on p. 101 as follows: "the letters of all the texts are thinly scratched." Here again, the reader does not find a single word which might justify this new appellation. In fact, there are dozens and dozens of texts which are "thinly scratched" next to texts with larger letters; ; e.g., JaS 72b, 78a, 129a, b, d, e, f, g, etc, and especially JaS 134b, 135b, 143a, 150b, c and 174b. Therefore, what is so "inevitably suggestive" about them except that they prove something that has been known for a very long time, viz. some writers prefer large letters and others small and even very small letters.

3 - No. 3. - This statement clearly proves that Macdonald's mind was definitely made up: regardless of the chronological order of all those texts ("no fewer than 7 inscriptoons," p. 101), his preconcieved idea STANDS. - His note 27 (pp. 133-134) is irrelevant to Saf. Even were all examples convincing - an they are not - they would still not prove that Macdonalds's case is admissible.

4 - No. 4. - This is Macdonald's case which he has been building up by biased innuendos. But, even were he to prove the posteriority of the "thinly scratched" texts, his case would still be nil because posteriority does not imply dependence.

G - Conclusion.

Macdonald's whole argumentation is squarely rejected as lacking good common sense, the basic honesty toward the texts and those who engraved them, and the elementary knowledge of Saf epigraphy. His general attitude very closely copies Winnett's literary production. Both authors have in common a passionate cult for preconcieved ideas and theories which prevent them from looking at the facts without distorting them and which impel them not to really pay attention to anything, texts or persons, that stands in the way of the ideas or theories of their choice.

Abbreviations

Ar = Arabic.

BaANU = M.A. Bāfaqīh, Āṯār wa-nuqūš al-ʿuqlat, Cairo, 1967. — vdrBrIT = A.van den Branden, Les inscriptions thamoudéennes, Louvain, 1950.

CIASA = Corpus des inscriptions et antiquités sud-arabes, 1, Louvain, 1977. — CIH = Corpus inscriptionum semiticarum. Pars IV: Inscriptiones himyariticas et sabaeas continens, Paris, 1889-1950. — CIS = Id.; Pars V: Inscriptiones saracenicas continens, Paris, 1950. — ClAS = V.A. Clark, A Study of New Safaitic Inscriptions from Jordan, Melbourne, 1979; reprint by "University Microfilms International," Ann Arbor, MI, 1983, with a few changes. For my review, cf. JaHAR, XIII, pp. 2-115. — ClNSI = V.A. Clark, "New Safaitic Inscriptions from Bakaka and Azraq," in Abr-Nahrain, 3 (1984-1985), pp. 14-21 (with facsimiles on pp. 15 and 18), and 2 plates of photographs. — ClSN = V.A. Clark, "Some New Pre-Islamic Arabian Inscriptions from Northern Arabia," in Abr-Nahrain, 17 (1976-1977), pp. 35-42 and 1 plate. — CoDRS = D. Cohen, Dictionnaire des racines sémitiques, Paris, 1970 foll.

DAFA = Dictionnaire arabe-français (langue classique et moderne). Arabic/French/English Dictionary, initiated by R. Blachère, M. Chouémi and C. Denizeau, Paris, 1967 foll. — DoSDA = R. Dozy, Supplément aux dictionnaires arabes, 2 vol., Leyde, 1967.

ErPAHN = A. Ernout, Pline l'Ancien. Histoire Naturelle, Livre XII, Paris, 1949.

FrLAL = G.W. Freytag, Lexicon arabico-latinum, 4 vol., Halle, 1830-1837.

GaPDRY = Gazetteer - Peoples Democratic Republic of Yemen, Washington, DC, 1976.

Gl = E. Glaser.

HaAIC = G.L. Harding, An Index and Concordance of Pre-Islamic Arabian Names and Inscriptions, Toronto, 1971. — HaCH = G.L. Harding, "The Cairn of Hani'," in ADAJ, 2 (1953), pp. 8-56 and pl. V-VII. — Ḥaḍr = Ḥaḍrami. — HaLi = texts published by G.L. Harding and E. Littmann, Some Thamudic Inscriptions from the Hashemite Kingdom of Jordan, Leide, 1952. — HaNEIP = J. Halévy, "Nouvel essai sur les inscriptions proto-arabes," in RS, 9 (1901), , 10 (1902) and 11 (1903). — HaNST = Saf inscriptions published by G.L. Harding, in ADAJ, 1 (1951), pp. 25-29 and 1 plate. — Hu = C. Huber.

Ja = Jamme. — JaGUR = A. Jamme, "The Grammatical Usage of the Safaitic Verb rʿy, to pasture," in AION, 31 (1971), pp. 21-40. — JaCMYE = A. Jamme, Carnegie Museum 1974-1975 Yemen Expedition, Pittsburg, PA, 1976. — JaIPSC = A. Jamme, "Inscriptions from Fifty Safaitic Cairns," in Orientalia, 48 (1979), pp. 478-528. — JaL = A. Jamme, Liḥyanite. — JaMAR = A. Jamme, Miscellanées d'ancient arabe, Washington DC, II (1971), III (1972), IV (1973), V (1974), VII (1974), VIII (1979), IX (1979), XII (1982), XIII (1983) and XIV (1985). — JaROx = A. Jamme, Review of OxSI, in Orientalia, 40 (1971), pp. 274-286. — JaRWiReAR = A. Jamme, Review of WiReAR, in JAOS, 92 (1972), pp. 519-529. — JaS = A. Jamme, Safaitic. — JaSaMA = A. Jaussen et R. Savignac, Mission archéologique en Arabie (mars-mai 1907), Paris, II (1914). — JaSICA = A. Jamme, "Safaitic Inscriptions from the Country of ʿArʿar and Raʾs al-ʿAnāʾīyah," in F. Altheim und R. Stiehl, Christentum am Roten Meer, Berlin, I, 1971, pp. 41-109 and 611-637. — JaSISA = A. Jamme, "Safaitic Inscriptions from Saudi Arabia," in Oriens Antiquus, 6 (1967), pp. 189-213. — JaSIMB = A. Jamme, Sabaean Inscriptions from Maḥram Bilqīs (Mārib), Baltimore, MD, 1962. — JaSN = A. Jamme, Safaitic Notes, Washington, DC, 1970. — JaSVW = A. Jamme, "The Safaitic Verb wgm," in Orientalia, 36 (1967), pp. 159-162. — JaT = A. Jamme, Tham. — JaTV = A. Jamme, "The Thamudic Verb ʾtm," in JAOS, 88 (1968), pp. 290-294. — JaUT = A. Jamme, The al-ʿUqlah Texts, Washington, DC, 1963. — JoGEL = H.S. Jones, A Greek-English Lexicon, Oxford, 1968. — JaGS = H.L. Jones, The Geography of Strabo, Books XV and XVI, Cambridge, MA, 1983. — JsaT = Jaussen-Savignac, Thamudic.

LaAEL = E.W. Lane, Arabic-English Lexicon, 8 vol., London, 1863-1893.— Liḥ = Liḥyanite(s). — LiSI = E. Littmann, Safaitic Inscriptions, Leyden, 1943. — LiTS = E. Littmann, Thamūd und Ṣafā, Leipzig, 1940. — LiZTI = E. Littmann, Zur Entzifferung der thamudenischen Inschriften, Berlin, 1904. — LP = texts published in LiSI.

Min = Minaean(s).

NA = North-Arabian.

OxSI = W.G. Oxtoby, Some Inscriptions of the Safaitic Bedouin, New Haven, CO, 1968; cf. my review in JaROx.

PiDPH = J. Pirenne, "Deux prospections historiques au Sud-Yémen (Novembre-Décembre 1981)," dans Raydān, 4 [1981] 1982, pp. 205-240 and 14 plates. — PiPH = J. Pirenne, "Prospection historique dans la région du royaume de ʾAwsān," in Raydān, 3 (1980), pp. 213-255 and 14 plates.

Qat = Qatabanian.

RaPNH = H. Rackham, Pliny. Natural History. II: Libri III-VI, Cambridge, MA, 1969. — RÉS = Répertoire d'épigraphie sémitique, Paris, V, 1928. — RycCRA = J. Ryckmans, "La chasse rituelle dans l'Arabie du Sud antique," dans al-Bahit. Festschrift Joseph Henninger, St. Augustin, 1976.

Sa = Sabaeica. — SA = South-Arabian. — SaAAL = H.A. Salmoné, An Advanced Learner's Arabic-English Dictionary, Beirut, 1972. — Sab = Sabaean. — Saf = Safaitic. — ScPES = W.H. Schoff, The Periplus of the Erythraean Sea, London, 1912. — SeSAH = R.B. Serjeant, South Arabian Hunt, London, 1976.

Tham = Thamudic.

VL = H. Van Lessen.

WH = texts published in WiHaI. — Wi = texts published in WiSIJ. — WiGS, I = H. von Wissmann, "Die Geschichte des Sabäerreichs und der Feldzug des Aelius Gallus," in H. Temporini und W. Haase, Aufstieg und Niedergang der römischen Welt, Berlin, II. 9. 1, [1976] 1977, pp. 308-545. — WiHaI = P.V. Winnett and G.L. Harding, Inscriptions from Fifty Safaitic Cairns, Toronto, 1978; cf. my review in JaIFSC. — WiHöBGS = H. von Wissmann und M. Höfner, Beiträge zur historischen Geographie des vorislamischen Südarabien, Wiesbaden, 1953. — WiReAES = F.V. Winnett and W.L. Reed, "An Archaeological-Epigraphical Survey of the Ḥāʾil Area in Northern Saudi Arabia," in Berytus, 22 (1973), pp. 53-100 and 13 plates. — WiReAR = F.V. Winnett and W.L. Reed, Ancient Records from North-Arabia, Toronto, 1970. — WiSIJ = F.V. Winnett, Safaitic Inscriptions from Jordan, Toronto, 1957. — WiSLTI = F.V. Winnett, A Study of the Lihyanite and Thamudic Inscriptions, Toronto, 1937. — WiTh = F.V. Winnett, Tham texts published in WiReAR. — WiZAG = H. von Wissmann, Zur Archäologie und Antiken Geographie von Südarabien, Istanbul, 1968.

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